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Biographical Memoirs

VOLUME XLII

MILISLAV DEMEREC

January 11, 1895–April 12, 1966

BY BENTLEY GLASS

IN THE YEAR, 1919, when a certain young Yugoslav agricultural student came to Cornell University to pursue graduate study in genetics under R. A. Emerson, the first great decade of American genetical studies was just drawing to a close. The ensuing four and a half decades, to the time of his death, have been identified with Milislav Demerec's own career and contributions as with few others. He was among the dozen or so geneticists whose work has made the United States preeminent in this biological science. His own contributions reflected not only great profundity in seizing upon the most important problems for investigation but also great flexibility in shifting from one area of significance to another, at the cost of abandoning tried techniques and acquiring skill in entirely new ones. Yet equally important to the advancement of science, along with his investigations and discoveries, were his gifts of scientific leadership and administration. No account of his life must ignore their value.

Milislav Demerec was born in Kostajnica, Yugoslavia, on January 11, 1895. He graduated from the College of Agriculture, Krizevci, Yugoslavia, in 1916 and held a position as Adjunct at the Krizevci Experiment Station until 1919, when he came to the United States to study at Cornell University. He received the Ph.D. degree in genetics from Cornell in 1923, having worked as assistant in plant breeding during several years of his graduate study. In 1923 he joined the staff of the Department of Genetics, Carnegie Institution of Washington, in Cold Spring Harbor, New York, as a Resident Investigator. He was appointed Assistant Director in 1936, Acting Director in 1942, and Director in 1943. From 1941 he was also Director of the neighboring Biological Laboratory of the Long Island Biological Association, which was later renamed the Cold Spring Harbor Laboratory of Quantitative Biology.

On June 30, 1960, Dr. Demerec retired from the directorship of the Department of Genetics of the Carnegie Institution of Washington, and joined the research staff of the Brookhaven National Laboratory as a Senior Staff Member. There he continued his program of research in bacterial genetics until the end of 1965. He then became Research Professor of Biology at C. W. Post College of Long Island University, and remained very active in research until his sudden death from a heart attack on April 12, 1966. A number of posthumous papers, completed by his colleagues, represent work actively in progress at the time.

He has twice been honored since his death, first by C. W. Post College, which named its science building the Milislav Demerec Science Laboratory; and most recently by the Cold Spring Harbor Laboratory of Quantitative Biology and the Carnegie Institution of Washington, which on June 5, 1968, jointly named in his honor the principal laboratory building constructed by the Carnegie Institution of Washington on the land of the Cold Spring Harbor Laboratory (held by the Wawepex Society).

Dr. Demerec held appointments as Research Associate of Columbia University (1943-1965), Consultant of the Brookhaven National Laboratory (since 1948), Visiting Professor at the Rockefeller Institute of Medical Research (1958-1960), and Consultant in Genetics and Cellular Biology at Adelphi

College. Besides giving lectures and seminars in many parts of the United States, Europe, Asia, Africa, and South America, he was instrumental in bringing numerous young foreign scientists and students to the United States to receive experience and instruction in methods of research developed here.

Dr. Demerec was a member of the Council of the 6th International Congress of Genetics, held in Ithaca in 1932; vice president of the 7th Congress, held in Edinburgh in 1939; member of the Permanent International Committee of the International Genetics Congress, 1939-1953; chairman of the Committee on Transportation for the 8th Congress, held in Stockholm in 1948; and a member of both the Organizing Committee and the Program Committee for the 10th Congress, held in Montreal in 1958. He served as secretary-treasurer (1935-1938), vice president (1938), and president (1939) of the Genetics Society of America; as treasurer (1933-1935), vice president (1947), and president (1954) of the American Society of Naturalists; as chairman of the U.S.A. National Committee of the International Union of Biological Sciences; and as member or chairman of many committees appointed by the National Academy of Sciences to study special problems of national and scientific importance. In particular, he served on the Committee on the Genetic Effects of Atomic Radiation, which made important reports in 1956 and 1960. He was chairman of the Section of Zoology and Anatomy of the Academy from 1958 to 1961.

Dr. Demerec was a member of the National Academy of Sciences (elected in 1946) and of the American Philosophical Society (elected in 1952). He was a fellow of the American Academy of Arts and Sciences, the New York Academy of Sciences, and the American Association for the Advancement of Science; a member of the council of the Radiation Research Society; and a member of many other professional organizations, including Sigma Xi, Phi Kappa Phi, the Society of American

Bacteriologists, the Society for the Study of Evolution, the American Genetic Association, the American Society of Zoologists, and the Botanical Society of America. He was honored for his scientific accomplishments by being awarded the Order of St. Sava by the Yugoslav government in 1935, and by honorary memberships in the Royal Danish Academy of Sciences and Letters, the British Genetical Society, the Academy of Sciences of Yugoslavia, the Genetics Society of Japan, the Society of Biology of Santiago, Chile, and the Faculty of Medicine of the University of Chile. In 1957 he received from Hofstra College the honorary degree of Doctor of Laws; in 1960 the degree of Doctor *honoris causa* from the University of Zagreb; and in 1961 the degree of Doctor of Science from Long Island University.

Dr. Demerec became a naturalized citizen of the United States in 1931. In 1921 he married Mary Alexander Ziegler, engineering graduate of Cornell University and teacher of science. Dr. and Mrs. Demerec had two daughters who have survived him, Mrs. Philip E. Hartman and Mrs. Neville Dyson-Hudson, both of Baltimore, Maryland; and five grandchildren.

Dr. Demerec's research spanned a period of forty-six years, and was directed toward a better understanding of the nature of genes, their structure and function, and their spontaneous and induced mutability. Demerec began his genetic investigations with a study of the basis of variable phenotypic traits. The striping of leaves, the variegations of maize seeds, and the virescence of seedlings that at first are albino but later turn green evoked in him a deep interest in the basis of somatic mosaicism. Suspecting somatic mutation to be a prevalent cause of such changes, he very early directed attention to this phenomenon, and shifted from maize to *Drosophila virilis* and to delphiniums as appropriate organisms for probing the nature of the factors that control mutation rates. His classic papers on the unstable genes of *Drosophila virilis* remain basic to an understanding of the mutation process. These genes exhibited different mutation rates at different stages of the life cycle and in different tissues.

A second period in Demerec's genetic investigations began in the early 1930s with the study of X-ray-induced mutations and deficiencies in Drosophila. Here, too, an interest in the role of gene mutation in ontogeny was evident, since one of his most important early contributions to this new area was the relation of induced deficiencies to cell lethality. With the introduction of salivary gland chromosome analysis, the work on deficiencies and other types of chromosome aberrations advanced rapidly, in collaboration first with Margaret Hoover, and later with B. P. Kaufmann, Eileen Sutton, and others. Demerec's interest in the factors controlling spontaneous mutability was not lost, however. From this period date also those extremely significant studies of the differences in mutability in various wild-type strains of *Drosophila melanogaster,* and the identification of a mutability-stimulating gene in the Florida stock.

By the 1940s Demerec was studying the effects of ultraviolet radiation and of neutrons in inducing gene mutations and chromosome aberrations, but the advent of World War II, with certain practical demands, brought about a shift of activity in his laboratory at Cold Spring Harbor. Of enormous practical importance was the work performed to induce, in Penicillium, mutations that greatly increased the yield of penicillin. Of greater long-range value were the parallel studies of mutations evoking resistance on the part of Staphylococcus to penicillin and other antibiotic agents or to drugs. A major new direction of genetics was initiated by Demerec and Ugo Fano in their 1945 paper on bacteriophage-resistant mutants in *Escherichia coli.* By the end of the 1940s Drosophila studies had been fully replaced by studies of radiations and chemical mutagens (including carcinogens) and their effects on the genes of bacteria, especially in relation to mutations promoting bacterial resistance. The latter research revealed two important principles to be followed in antibiotics therapy. These drugs should be administered in high enough initial doses to prevent the occurrence of "second-step" (highly) resistant bacterial mutants; and they should be used in combination rather than singly, since a mutant that is resistant to one antibiotic has a very small chance of being resistant to another one also. This work in turn led, in the 1950s, to the studies that were to occupy Demerec until the end, namely, the investigation of recombination and the fine structure of the gene in Salmonella, the agent causing mouse typhoid.

By special techniques of transduction and hybridization, Demerec analyzed various genes that control the synthesis of amino acids and purines in Salmonella. Independently, and in collaboration with Philip Hartman and others, Demerec analyzed the functional organization of the bacterial chromosome and discovered a remarkable parallelism between the sequence of functional units and the sequence of biochemical steps in a chainlike synthesis. It became evident that the gene is a complex structure, rather than the basic unit of heredity as was formerly supposed. Smaller units, or mutational sites, can be distinguished within it, and their relative positions determined. It appears that these sites, too, tend to be grouped together according to similarity of function.

Aside from his earliest scientific studies while a graduate student, and his years following retirement, which were spent at the Brookhaven National Laboratory and at C. W. Post College, all of Demerec's long career was identified with Cold Spring Harbor, from that first day in 1923 when he arrived as a fresh Ph.D. to join the staff of the Department of Genetics of the Carnegie Institution of Washington, to the day in 1960 when he retired as Director of both the Carnegie Institution's

Department of Genetics and the Long Island Biological Laboratory. In that span of nearly forty years of activity, Demerec had made Cold Spring Harbor a worldwide focus of inspiration and leadership in genetics. This role began when he became Director of the Biological Laboratory in 1941, and in the following year Acting Director of the Carnegie Institution's Department of Genetics. In 1943 he became the Director of the Carnegie Department, and the close and fruitful collaboration of the two neighboring biological institutions started them toward the fusion that eventually took place. The outstanding series of Cold Spring Harbor Symposia in Quantitative Biology which he organized from 1941 to 1960 brought scientists from all over the world to engage in relaxed, informal conversations on the sandspit and the veranda of Blackford Hall, as well as in the formal sessions where interesting and sometimes epochal papers were presented and discussed. From 1941, when the symposium on "Genes and Chromosomes" was held, until the year of his retirement, it was chiefly the foresight and wisdom of Milislav Demerec in choosing subjects of most timely interest, selecting participants of worldwide eminence, organizing the program, and editing the proceedings that made Cold Spring Harbor in June the mecca of genetic biology, where genetics, evolution, and biochemistry came together in fruitful interaction.*

Stimulating contacts spread through the summer as visiting research workers exchanged ideas with each other and with members of the permanent staffs of the Biological Laboratory and the Carnegie Department. At least one visitor can testify that he never worked harder than during those summers and never had a better vacation for himself and his family than in those years when Cold Spring Harbor was a summer home and laboratory. By judicious appointments to the research staffs of the two institutions, Demerec kept the spearheads of investigation probing into significant new fields. Especially noteworthy was the arrival of A. D. Hershey in 1950. Cold Spring Harbor had already become at that date a center of the phage group. Summer meetings had led in 1945 to the establishment of a special summer course for instruction in the theory and techniques of genetic investigation with bacterial viruses. A parallel summer course in the genetics of bacteria was started in 1955. These and later additions to the summer courses have added undying importance to Cold Spring Harbor as a biological center, for here germinated much of the work that flowered in the present effulgence of microbial and viral genetics throughout the world.

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* It is characteristic of Demerec's innate modesty that he did not list these volumes in his bibliography, but in simple justice this has been done by the author of the present memoir.

Somehow, year after year, Demerec made this possible on a financial shoestring. The Carnegie Institution provided not only new laboratories for its own staff but also a grant to the Biological Laboratory for the construction of a handsome auditorium suitable for future Cold Spring Harbor symposia and other summer meetings. Grants were obtained to support the permanent staff of the Biological Laboratory and the summer courses, and to maintain the charming but ancient buildings. Year after year local friends of the Cold Spring Harbor community were encouraged to help maintain the Laboratory through their gifts. The scientific community throughout the United States was canvassed. In mysterious ways, and with real courage, for two decades Demerec found the means not only to keep the summer programs and the year-round research going, but to expand them in significant directions of the new molecular biology that was springing into being.

Demerec served the scientific community in many ways. In addition to the large number of scientific organizations, congresses, and committees he aided, his editorial activities were very great. He was responsible for the publication of the *Cold*

Spring Harbor Symposia on Quantitative Biology. In 1934 he started *Drosophila Information Service.* This was the first of the professional newsletters and information services in genetics. He established the Drosophila stock center at Cold Spring Harbor in the Carnegie Department of Genetics, the first of such stock centers to maintain the important experimental stocks of Drosophila and to supply them to Drosophila workers around the world. He edited a basic handbook for Drosophila, *The Biology of Drosophila.* Together with B. P. Kaufmann, he wrote the *Drosophila Guide,* a handbook of enormous usefulness to all geneticists and other workers with that exemplary organism. The booklet has gone through seven editions. Demerec founded the series *Advances in Genetics* (Academic Press) and served as editor for nine volumes, from 1947 through 1958; he also served on the editorial boards of several genetical journals. Yet in the end those of us who knew him well will remember him as the characteristic spirit and impulse of Cold Spring Harbor, a nerve center of modern genetics.

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KEY TO ABBREVIATIONS

Am. J. Bot. = American Journal of Botany
Am. Naturalist = American Naturalist
Anat. Record = Anatomical Record
Carnegie Inst. Wash. Publ. = Carnegie Institution of Washington Publication
Carnegie Inst. Wash. Year Book = Carnegie Institution of Washington Year Book
Cold Spring Harbor Symp. Quant. Biol. = Cold Spring Harbor Symposium on Quantitative Biology
J. Bacteriol. = Journal of Bacteriology
J. Heredity = Journal of Heredity
Microb. Genet. Bull. = Microbial Genetics Bulletin
Proc. Internat. Congr. Genet. = Proceedings of the International Congress of Genetics
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Records Genet. Soc. Am. = Records of the Genetics Society of America

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ALPHONSE RAYMOND DOCHEZ

April 21, 1882–June 30, 1964

BY MICHAEL HEIDELBERGER, YALE KNEELAND, JR., AND KATHERINE MILLS PRICE

THERE ARE, among scientists, some who prefer to work mainly with their hands, gathering, recording, and disseminating new facts. At the other extreme are the thinkers, who listen, read voraciously, and synthesize the handiwork of the first group into theories that show the way to further progress. Alphonse Raymond Dochez began his scientific career as a simple worker in the laboratory, but matured rapidly into the rare combination of the gatherer of facts and the thinker. In addition, he possessed the gift of articulate expression, and as he loved to talk, he radiated inspiration and ideas to an admiring and ever-expanding circle of colleagues.

Dochez was born in San Francisco, of Belgian descent, on April 21, 1882. His parents were Louis and Josephine Dietrich Dochez. Little is known of his early childhood except that his mother and her several small children went to Indianapolis where Alphonse received his pre-college education. Eventually they settled at his mother's parents' farm in Harford County, Maryland. Here young Dochez fished in the streams emptying into upper Chesapeake Bay, hunted, and wandered in the hills. The while his mother instilled in her son a strict conformity to the rituals of the Catholic church as well as a broad tolerance and an undeviating kindliness. For his formal education he commuted daily to Baltimore on the Baltimore and Ohio Railroad, which passed near his home. After acquisition of a B.A. degree at Johns Hopkins in 1903, he was still undecided as to a career but began the study of medicine at the insistence of an eccentric and visionary uncle who soon after departed for Venezuela and was never again heard from. At this period the family operated a small cannery on the farm, putting up corn and other products.

Dochez received his M.D. degree in 1907. While he apparently enjoyed his undergraduate medical education, it is uncertain when his bent as an investigator began to develop. It is said that at about Christmas-time of his first year in medical school he was informed that in all likelihood he would fail anatomy. Shocked by this information, he expressed incredulity. The professor replied that he had never seen him in the laboratory on Saturdays for the entire autumn term. "But," interjected Dochez, "I always go shooting on Saturday." The conversation must have had some effect, for he did pass anatomy, but his career as a hunter was curtailed if not terminated.

Immediately after graduation he entered the laboratory of pathology at Johns Hopkins, where he spent a year studying the effects of an iodine-free diet upon animals. Hearing by chance that the newly organized Rockefeller Institute for Medical Research might have an opening, Dochez went to New York for an interview with Dr. Simon Flexner, the director, since no young aspiring unknown could be appointed without Dr. Flexner's personal approval. Dochez evidently passed muster, for he was given a fellowship in pathology at the Rockefeller Institute with Dr. Eugene Opie, who, at this writing, was again at work there in spite of his great age. Dochez referred to his year or two with Opie as "only too short." The results of his studies with Opie were included in four papers on the activation of trypsin and other enzymes. Many years later Dochez looked back with great interest on this pioneering work with proteolytic enzymes in the liver and regretted that the findings did not seem to have been much heeded.

In 1910 the hospital of the Rockefeller Institute was opened under the directorship of Dr. Rufus Cole. Dochez was offered an assistant residency, and also the position of bacteriologist to the hospital. He remembered years later with some amusement having asked Hideyo Noguchi, who was also at the Institute, if one could possibly become a bacteriologist in three weeks. Whether Noguchi replied affirmatively is not recorded, but in any event Dochez became a bacteriologist and remained a microbiologist to the end of his life. His appointment at the hospital of the Rockefeller Institute was also a clinical one, and he remained as assistant resident and resident for five years, during which he was a major participant in the famous studies which were instituted there on lobar and bronchial pneumonia. Broadly speaking, Dochez's contributions may be summarized as follows: he established a biological classification of pneumococci into specific types. He and Oswald T. Avery discovered the soluble specific substance which confers type-specificity upon the pneumococci and yields precipitates with the homologous antiserum, showed that the substance was of capsular origin, and demonstrated its frequent presence in the blood and urine of patients during the acute stage of pneumonia. Dochez loved to tell how he and Avery reasoned that, since the soluble substance occurred in the blood of patients gravely ill with pneumonia, being soluble and diffusible, it should also pass through the kidneys and appear in the urine. They accordingly sent for a sample of the urine of a patient with a severe Type II pneumonia and set it up against some Type II antipneumococcal serum. No precipitation occurred, so they sat around glumly looking at the clear liquid in the test tube, wondering how and why their reasoning could have been wrong. Finally, one of them walked over to the vase of urine and looked at the label: it was from another patient! Needless to say, the test was positive when the proper specimen was obtained.

Dochez, Avery, and their co-workers demonstrated the importance of type-specific antibodies in the mechanism of recovery from pneumococcal pneumonia. This led directly to the production of type-specific antipneumococcal horse serum, and they demonstrated the efficacy of this serum in the treatment of Type I pneumonia. Type-specific antisera, especially those later raised in rabbits, and, more particularly, the purified globulin of the rabbit antisera, remained the only effective therapy of pneumococcal pneumonia until the introduction of the sulfa-drugs and penicillin. Dochez's studies of the pneumococcus lasted for about nine years, and this body of work fully warrants the application of that somewhat overworked word "classical."

Dochez continued his clinical studies of respiratory diseases as a major in the Medical Corps in World War I and made many trips in efforts to control outbreaks. In 1919 he returned to the Johns Hopkins Medical School as Associate Professor of Medicine. It was here that he began his studies of the streptococcus and its relationship to scarlet fever which he continued after he joined the staff of the College of Physicians and Surgeons of Columbia University in 1921 as Professor of Medicine. At the time Dochez started his studies of scarlet fever, the streptococcal etiology of that disease had not been established and, in many quarters, was regarded with skepticism. Dochez showed that there was a direct relationship between streptococcal pharyngitis and scarlet fever, and that most of the strains of streptococci isolated from patients with scarlet fever belonged to a single, specific type.

Dr. Franklin M. Hanger, Jr., later also Professor of Medicine at the College of Physicians and Surgeons of Columbia University, writes of this work: "I remember well when

Dochez injected melted agar under the skin of a young pig and inoculated it with his Pr 1 strain of streptococcus. A few days later, at about eleven o'clock at night, he called me excitedly on the phone to come up to his laboratory. There was his little pig, as rosy as a boiled lobster! The logical next step was to immunize a horse by the same procedure, and this Doh, as he was familiarly called, started in the stables of the Rockefeller Institute." The antiserum, injected intradermally, blanched the rash of scarlet fever, and given parenterally, had a striking therapeutic effect. An epidemic of scarlet fever in New Haven provided the first large-scale opportunity to test the serum, and its beneficial effects were enthusiastically reported by a number of observers.

Unfortunately, Dochez's studies of streptococcal infections were terminated by a legal decision handed down in favor of George F. and Gladys H. Dick of Evanston, Illinois. They had obtained British patent 243675, dated November 28, 1924, and U.S. patent 1,547,369, dated July 28, 1925, making broad claims as to the isolation of streptococci specific to scarlet fever, the preparation of a scarlatinal toxin, the injection of animals to obtain an antitoxin, and the antitoxin itself. Dochez had earlier taken out British patent 232181, dated April 14, 1924, but his U.S. patent 1,585,090, dated May 18, 1926, and assigned to the Presbyterian Hospital of New York, was issued after that of the Dicks. This apparently served as the basis for the decision of infringement of their patent, even though Dochez's ingenious method of production of the antitoxin in animals was not specifically mentioned in the Dicks' patent.

At the enforced conclusion of his studies of scarlet fever, Dochez shifted to the last of the three major fields of his endeavor. This was the common cold, or perhaps, more literally, common upper respiratory infection. His studies here were pursued in an orderly and systematic fashion. Dochez and his younger collaborators Gerald S. Shibley, Alvin F. Coburn, Franklin M. Hanger, Jr., Yale Kneeland, Jr., and Theodore Badger first studied the bacterial flora of the upper respiratory tract and convinced themselves that these were not of primary etiological significance. Similar studies failed to incriminate the gram-negative filter-passing anaerobes. Then, using first chimpanzees and later *homo sapiens* as experimental animals, Dochez showed that typical colds could be produced by exposure of the subjects to bacteria-free filtrates, thus indicating a viral etiology. Attempts at cultivation of these filtrable agents showed promise, but the techniques of viral cultivation of the period were not adequate for the indefinite propagation of such infectious agents.

At the end of the 1930s Dochez found himself more and more involved in administrative work. In 1940 he was appointed chairman of the Department of Bacteriology at the College of Physicians and Surgeons, a post which he held for nine years, although he escaped as often as possible to return nearly every day for afternoon tea in his old haunts in the Department of Medicine. Here one was always privileged to drop in for half-an-hour's stimulating discussion with "Doh" of a "hot" problem or one that merited attention. Dochez retired in 1949, with the title of John E. Borne Professor of Medical and Surgical Research Emeritus, and continued his active interest in biological problems until nearly the moment of death. One of the last of his intellectual achievements was a profound and perceptive study of the problems of carcinogenesis, and he would discourse remarkably energetically upon these with visitors to his room in the Harkness Pavilion of the Presbyterian Hospital in New York. There he died on June 30, 1964, at the age of eighty-two. He had suffered progressive physical incapacitation for several years and had lived perforce a very secluded life.

It can be seen that Dochez's efforts resulted in major contributions in three distinct and separate areas. As the late Oswald T. Avery remarked when presenting to him the Kober Medal of the Association of American Physicians, "Throughout his studies there is a unique continuity of thought centering in the dominant problem of acute respiratory diseases. The results of his work are not random products of chance observation. They are the fruits of years of wise reflection, objective thinking, and thoughtful experimentation."

His long-time co-worker, Katherine Mills, now Mrs. H. Stewart Price, writes: "I worked for Dr. Dochez as a laboratory technician for over thirty years. We on his laboratory staff felt we were working *with,* as well as for, him. He encouraged us to consider ourselves his research assistants. Our names were included in publications; indeed, on many of Dr. Dochez's papers, the order of names frequently began with that of a younger colleague or a lesser member of the team. I came in the autumn of 1924, when he was starting his research on the common cold. In the first interview he outlined the general strategy, planning to begin with an intensive study of the role of the usual respiratory pathogens and a separate consideration of the then recently described gram-negative filter-passing anaerobes. If, as Dr. Dochez thought even then, these studies showed that bacteria could not be implicated as the primary cause, he planned the study of viral agents. This was to be done by transmission experiments in carefully quarantined human beings and apes. If, and when, a viral etiology was established we would attempt to isolate and cultivate the agent, always with particular attention to finding a suitable small animal or a definitive laboratory test that would make possible large numbers of experiments. It was exciting to see the broad concept of the problem behind the technical details with which I would be concerned.

"This reasoned approach was characteristic. We were encouraged to look for new phenomena or hopeful leads, but to keep to the main purpose and not wander down rosy bypaths. We were also urged to dig exhaustively into the archives. Dr. Dochez himself had an encyclopedic knowledge of the literature, and was scrupulously exact in citing the work of others.

"We were fortunate in the laboratory, for it was here that we saw Dr. Dochez frequently and on an informal basis. His lifetime interest in laboratory work never abated, despite the press of clinical and administrative demands. I think he enjoyed even the smell of a laboratory. It was amazing to see how he knew the smallest details of the daily routine. When he joined in the actual work his technique was a delight to watch. His instant grasp of a problem was of inestimable value. I well remember that when we ran into the agglutination of red cells by the pneumonia virus of mice we were baffled by the fact that, although the agglutination was striking in blood oozing from a cut surface of the lung, it could not be demonstrated in ground suspensions. This was during World War II and Dr. Dochez was not in the laboratory as often as usual. He came in one day, looked at a few Petri dishes with the cut lungs, said immediately that the agglutination was very definite, that something must be inhibiting it in the suspension, and to try inactivation with heat. With that he sauntered out, having solved the whole difficulty.

"We had tea in the laboratory every afternoon. While the basic group of our 'Tea Club' was small, it was extraordinary how many doctors developed a sudden thirst if they happened to catch a glimpse of 'the Boss' in his usual chair by the window. They would come in, quite casually of course, for discussions of their own work, and his interest and knowledge were always given with generous enthusiasm. Talk was generally along scientific lines, but also ranged from world affairs to the latest flurry in the stock market or the standing of the baseball teams. Dr. Dochez did not give the impression of being an outdoorsman. He once told me that he had taken a camping trip, and thereafter it was years before he would stand in the shade of a tree! He was, however, an excellent shot in a duckblind, and his golf game was in the 70s. I believe he was one of the few golfers to have two holes-in-one to his credit. These feats delighted him almost as much as a successful experiment.

"Through all these years of spontaneous and uncensored conversation I never heard Dr. Dochez make an unkind remark about anyone or their work. He might keep silent, but he did not criticize, even with ample provocation. The loyalty and devotion of those who worked for Dr. Dochez were universal. This is all the more remarkable as he was very reserved and somewhat remote. But young and old, men and women, from porters to professors, regarded him with admiration and trust. His absolute lack of pettiness was reflected in the smooth functioning of the department. When one rather bitter feud smoldered in the lower echelons, personal animosities ran high, but both sides were agreed on at least one thing: the Boss should not be disturbed nor the work affected. He, of course, was aware of the friction—he didn't miss much—but ignored it serenely, and eventually harmony was restored. He had a way of bringing out the best in people. We all realized we were privileged to work for a great man. His own work will stand for all time and that of many others was stimulated by him."

During the years of World War II, and for some time thereafter, Dochez performed major services for the government. These included active membership in the Office of Scientific Research and Development and on the Board for Coordination of Malarial Studies, the Board for Control of Influenza and Other Epidemic Diseases, the Hoover Commission on the reorganization of the executive branch of the government, and a number of others. His effective work on these commissions earned him the Medal of Merit. In addition to this and the Kober Medal, he was also the recipient of the Medal of the New York Academy of Medicine. He was a member of numerous learned societies, and served as president of the Association of American Physicians, the American Clinical and Climatological Association, and the American Association of Immunologists. He became a member of the National Academy of Sciences in 1933.

Dochez received an honorary degree from Yale University in 1926. Other honorary degrees came from New York University in 1925 and Western Reserve in 1931.

Dochez was never greatly impressed with the type of clinical investigation which consists in the acquisition of a new machine of some sort and then the recording of routine measurements. Rather he preferred to tackle a major problem from a new point of view. He always maintained that if one had a thorough and selective knowledge of the literature one could find clues. These clues one sorted in one's head until some sort of tentative hypothesis emerged and then one devised a simple experiment to test it. If this yielded what he used to speak of as an "indication," then one repeated the experiment, and if the indication was still present one devised a larger and more formal series of experiments to establish a convincing theory.

It goes without saying that such a man was constantly being consulted by his colleagues about their researches, even when these were in fields widely separated from his own. And seldom did one of them emerge from such a conference without a feeling that his thinking had been clarified and the problem better defined. If the field were related to Dochez's own, his suggestions were often miraculously helpful.

The foregoing paragraphs have indicated some of the high points of Dochez's career and have attempted some notion of his accomplishments and his *modus operandi*. But a memoir would be incomplete without further description of the man himself, his character and personality. To begin with, he was extraordinarily handsome and endowed with a very good figure. Although he never discussed clothes, it was perfectly obvious that he liked them and had developed to a very high point the art of fastidious and elegant dressing. Seeing him on Fifth Avenue one would never in the world suspect that one was looking at a distinguished biological scientist. He was widely read and had a knowledgeable appreciation of music and the graphic arts. As an extremely attractive and eligible bachelor he was much sought after for many years in New York society. Most conspicuously, however, he was *not* a name-dropper, and the only notion of exalted social circles in which he moved came if one happened to bump into their periphery oneself. A lady once remarked to him that for a scientist he seemed to spend a good deal of time in various boxes at the opera. His rejoinder was that, while he might seem to be in a state of suspended animation, this was, in fact, the time in which he did much of his most solid and productive thinking. And it would appear that not infrequently he returned from the Metropolitan Opera, discovered Dr. Avery, with whom he shared an apartment, reading quietly in bed, and then would sit down in full evening dress and with vast animation describe to his old friend some of the illuminating thoughts on the subject of microbiology which had occurred to him during the second act of *La Traviata,* or whatever the evening's opera had been. Dochez was a devout Roman Catholic although he never discussed religion. If unable to escape from gossip, he might listen to it, but he never repeated what he heard. For him to have made an unkind or invidious remark about one of his associates or acquaintances would have been unimaginable. Occasionally, under great stress, he might show faint symptoms of annoyance, but these melted away with a laugh. If he ever lost his temper no one saw him do it. He was deeply uninterested in the more squalid aspects of life.

All those who saw anything of him in the last few years of his life were impressed by his unflagging passion for biological speculation, and it is noteworthy that two years before his death, at a time when he was already afflicted with partial motor aphasia, he composed an extraordinarily intelligent "Letter to the Editor" on the nature of the malignant cell which was published in the *Lancet*.

Those who knew Dochez well have reserved for him a special and unique place in their memories. There was no one like "Doh," nor does it seem likely to his old friends that there will ever be another comparable figure.

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KEY TO ABBREVIATIONS

Bull. Johns Hopkins Hosp. = Bulletin of the Johns Hopkins Hospital
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Exp. Med. = Journal of Experimental Medicine
Monogr. Rockefeller Inst. Med. Res. = Monographs of the Rockefeller Institute for Medical Research
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
Trans. Assoc. Am. Physicians = Transactions of the Association of American Physicians

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William Francis Gibbs

WILLIAM FRANCIS GIBBS

August 24, 1886–September 6, 1967

BY WALTER C. BACHMAN

FEW MEN HAVE SO WHOLEHEARTEDLY dedicated a lifetime to a single objective or derived so much enjoyment from the great effort devoted to achieving their goals as William Francis Gibbs.

Born August 24, 1886, in Philadelphia, Pennsylvania, son of a successful financier, William Warren Gibbs, and of Frances Ayres (Johnson) Gibbs, he received his primary and secondary education in the DeLancey School there, graduating in 1905.

An early interest in ships was undoubtedly stimulated when, as a boy, he had the opportunity to witness a ship launching at the Cramp shipyard in Philadelphia. This interest was confirmed and strengthened by a number of transatlantic crossings he made with his younger brother, Frederic, starting in 1901 with a trip on the White Star Liner *Oceanic,* the largest transatlantic passenger ship at that time. Later crossings were made on the *Celtic* and the *Lusitania,* outstanding ships of their day, and in 1907 on the maiden voyage of the *Mauretania,* which held the transatlantic speed record for over twenty years.

In 1906 he entered Harvard, where with characteristic individualism he did not pursue a formal curriculum leading to a degree but selected a combination of courses, largely scientific, in his range of interest. His leisure was devoted to reading technical publications dealing with ship design and construction. During this period he made an extensive study of all available information on the newest warships of the British Navy, which was preeminent at that time.

His studies at Harvard were completed in 1910. Even though no degree was conferred, the Harvard Chapter of Phi Beta Kappa made him an honorary member in 1945.

On the advice of his father, who considered engineers poor businessmen, he entered the Columbia University Law School in 1911 and received a Bachelor of Law degree in 1913. At the same time, he did graduate work in economics for which he received the degree of Master of Arts, also in 1913.

During this period his father suffered financial reverses and William Francis took a position in the law office of William Osgood Morgan in New York. His vision of great ships remained, however. For the next two years, while working at law in New York during the week, he journeyed each weekend to the family home in Haverford, Pennsylvania. There he spent all of his spare time together with his brother, Frederic Herbert Gibbs, investigating the possibility of designing a high-speed transatlantic passenger liner, 1,000 feet long. By May 1915, he was so encouraged by the results of this study that he gave up all other work so that he could devote full time to this project.

By January 1916, their design had progressed sufficiently so that the brothers presented it to Admiral David W. Taylor, then Chief Constructor of the U.S. Navy, and to the Hon. Josephus Daniels, Secretary of the Navy.

Encouraged by these men, they continued their efforts and, in June 1916, presented their design to Mr. P. A. S. Franklin, President of the International Mercantile Marine Company. Their proposal at this time included the development of a new port at Montauk, Long Island, with fast boat trains running to New York in order to reduce the total travel time to a minimum.

Mr. Franklin introduced the Gibbs brothers to Mr. J. P. Morgan, who was so impressed that he undertook the financing of further development of the design, including the necessary model testing.

Work on this project was interrupted by the entry of the United States into World War I in April 1917. W. F. Gibbs was appointed Assistant to the Chairman of the Shipping Control Committee of the General Staff of the U.S. Army and after the war was Assistant to the Chairman, U.S. Shipping Board on the American Commission to Negotiate Peace, in which capacity he attended the Peace Conference at Versailles.

In 1919 he was appointed Chief of Construction of the International Mercantile Marine Company, for whom he planned and supervised the conversion of the S.S. *Minnekahda* from a wartime transport to a third-class passenger and cargo ship for the New York to Hamburg service.

The great new German passenger liner *Vaterland,* which had made her maiden voyage to New York in May 1914, had been trapped there by the opening of World War I and had been seized by the United States government for use as a troop ship in 1917, when it was renamed the *Leviathan.* At the end of the war, the United States Shipping Board decided to convert the *Leviathan* for passenger service and the Gibbs brothers were asked to organize an independent firm to supervise this work.

Accordingly, in February 1922, Gibbs Brothers, Inc. was organized, with William Francis Gibbs as president.

The *Leviathan* had been built by the German shipyard Blohm & Voss, which held the detailed plans needed for the conversion and maintenance of the ship. The shipyard demanded one million dollars for a set of these plans—an exorbitant price at that time. Never one to submit to pressure, Gibbs immediately assembled a team of experienced men who completely measured the ship, including the internals of the main machinery, and drew their own plans at a considerable saving of money.

The ship was converted for luxury passenger service by the Newport News Shipbuilding & Dry Dock Company, under Gibb's supervision, with the care and attention to detail for which he soon became famous. The maiden voyage, which began on July 4, 1923, and several voyages thereafter were made under the supervision of Gibbs Brothers, Inc., acting as operating agents for the Shipping Board. With the successful completion of this great project, the reputation of the new firm was established.

Other commissions followed and the firm supervised the conversion of a number of ships released from their wartime service to suit commercial requirements.

The next great opportunity came in 1924 when Gibbs Brothers, Inc. designed and supervised the construction of the S. S. *Malolo* for the American-Hawaiian Steamship Company, a subsidiary of the Matson Navigation Company. This ship was built by William Cramp & Son in Philadelphia, the shipyard where William Francis witnessed his first launching as a boy.

The *Malolo* was the largest and fastest passenger liner built in the United States up to that time. Its design embodied a number of advanced safety features, including an improved arrangement of watertight compartments, connected, where necessary, through hydraulically operated sliding doors controlled from the bridge.

Few ship designers have had their ideas so promptly and dramatically tested. During the trial trip, a Norwegian freighter, the *Jacob Christensen,* struck the *Malolo* almost amidships while she was in a dense fog off Nantucket. Similar damage had previously caused the sinking of several passenger ships, most notably the *Empress of Ireland,* with great loss of life. The *Malolo* was disabled but was towed back to port with no injury to anyone on board. Gibbs's safety features were widely acclaimed and his reputation as a designer was confirmed, as was his resolve to maintain only the very highest standards of safety at all times.

Other work followed and, in 1929, the firm of Gibbs Brothers, Inc. was succeeded by Gibbs & Cox, Inc., a new firm organized to include Daniel H. Cox, an outstanding yacht designer of that period. This association permitted the Gibbs talent to be applied to several outstanding yachts, including the *Savarona,* the largest steam yacht ever built and the first to include the highest safety standards for merchant ships; and the great sailing yacht, *Sea Cloud,* 316 feet over-all length, with the number and arrangement of sails of a full-rigged ship.

Although this was a lean period for shipbuilders and designers in America, other important projects handled by the Gibbs firm included the design and supervision of construction of the four popular Grace Line passenger ships of the *Santa Rosa* Class and the S. S. *Angelina* and *Manuela* for A. H. Bull & Company. The latter two ships were the first general cargo vessels built in the United States in almost a decade and were considered a great advance in many respects in both hull and machinery.

The Grace Line ships were significant in another way, in that their machinery included solid rotor impulse turbines, steam conditions high for that day, and other features which provided the foundation for the major advances which Gibbs later made in the machinery used in the U.S. Navy.

Gibbs's outstanding knowledge of passenger ships was then devoted to the design and supervision of the construction of the S. S. *America.* This was the largest merchant ship built in this country up to that time and had safety features, including fire resistant construction, to a degree unequaled by any other passenger ship of the period. Completed in 1940 by the Newport News Shipbuilding & Dry Dock Company, she could not be used by the United States Lines in her intended service because of the war in Europe. After a luxury cruise to California, she returned to Newport News where her luxurious furnishings were removed and she was reoutfitted as the troop transport U.S.S. *West Point.* Refurbished at the end of World War II, she became a popular ship in the transatlantic service for which she was designed.

In 1933 the United States Navy, which had built almost no new ships since World War I, started a new construction program. Three shipyards, United Dry Docks, Inc, Federal Shipbuilding & Dry Dock Company, and the Bath Iron Works Corporation, undertook to build destroyers to a single design developed by W. F. Gibbs.

This program was particularly significant in two ways. It represented the first step in the development of modern, rugged, and more efficient steam propulsion machinery for naval ships. This type of machinery, further developed through a series of destroyer types, also of Gibbs design, was used to power practically all steam-driven combatant ships in the U.S. Navy built during World War II, including destroyers, cruisers, battleships, and aircraft carriers.

The work which was done on this first destroyer class, known as the U.S.S. *Mahan* or DD364 Class, also permitted the Gibbs firm to develop the methods and the central design and purchasing organization which made possible the large-scale shipbuilding programs of World War II.

This first new destroyer design was followed by many others. The destroyer classes known as DD381, 394, 397, 409, 423, 429, 432, and 437, all developed by Gibbs & Cox, Inc., permitted a steady evolution and refinement which resulted in the highly efficient designs known as the DD445, 453, and 692 Classes, which were the backbone of the World War II fleet.

In the DD381 and 394 Classes, Gibbs raised the steam pressure to 600 PSIG. This was considered a radical step at that time. Although it had the support of many farsighted naval officers, it also aroused the fears and opposition of many others. The "Battle of High Pressure–High Temperature Steam" raged for several years until the new ships demonstrated by their performance in service that they were not only more efficient but also more reliable than their predecessors.

During this period the firm also developed the working plans for light cruisers, Army transports, and the very successful *Wind* Class of icebreakers. The last-named, with diesel electric propulsion, bow propellers, and rolling tanks, were then the most powerful and effective in the world. Seven ships of the *Wind* Class were built. All are still in service.

In 1940 the British government sent a purchasing mission to survey the busy American shipyards and order some cargo ships. The head of the mission approached W. F. Gibbs and said that his government wanted twenty ships. The reply was, "You don't need them." Gibbs then explained that if Britain was within twenty ships of winning the war she had won already. If not, she needed many more ships. The British then placed an order for sixty *Ocean* Class ships to be built in two new shipyards. From these ships was evolved the design of the great fleet of Liberty ships which carried so much of our military cargo during the war years.

The first ship of the British *Ocea*n Class fleet was finished ten months and a day after the design was begun, a startling achievement.

The Gibbs brothers' experience with the U.S. Shipping Control Committee during World War I had shown very clearly the confusion and delays which could result if each shipbuilder and equipment manufacturer was permitted to proceed in his own way. With the design and purchasing organization developed for his naval work, W. F. Gibbs was in a position to develop designs suitable for mass production and to insist on the degree of standardization of equipment produced by suppliers to make the large wartime shipbuilding programs possible.

Knowing this background, the United States Maritime Commission requested Gibbs & Cox, Inc. to prepare the working plans and do the procurement for the first 312 Liberty ships. The first of these ships went on trial on December 26, 1941, soon after Pearl Harbor, and only nine months after the design was started. A total of 2,628 ships was built to this design in many shipyards before this program was ended.

As the United States mobilized for the war effort, the War Production Board was established and Charles E. Wilson, Vice Chairman for Production, requested W. F. Gibbs to become Controller of Shipbuilding to coordinate the ship construction programs of the Navy, the Army, and the Maritime Commission. This appointment was made with an arrangement which permitted Gibbs to control United States shipbuilding policies while at the same time he was freed of administrative detail so that he could continue to supervise the very important activities of his own company.

During this period he served as Chairman of the Combined Shipbuilding Committee (Standardization of Design) of the Combined Chiefs of Staff. He was also Special Assistant to the Director, Office of War Mobilization, and representative of the Office of War Mobilization on the Procurement Review Board of the Navy.

The technical organization developed for the prewar naval programs was rapidly expanded and under Gibbs's direction his firm produced the designs or working plans for over 63 percent of all ocean-going merchant ships and 74 percent of all naval vessels built in this country during the war years.

These wartime designs included the following:

BDE Class Escort Vessels, 1941 (97 ships)
DE(DET) Class Escort Vessels, 1942 (72 ships)
LST1-490 Class Landing Ship Tank, 1942 (441 ships)
LSD1 Class Landing Ship Dock, 1942 (15 ships)

GR121—U.S.S. *Mackinaw*—Icebreaker, 1942 (1 ship)
AOG1 Class Navy Gasoline Tankers, 1942 (11 ships)
DE(FMR) Class Escort Vessels, 1942 (85 ships)
DE(WGT) Class Escort Vessels, 1943 (81 ships)
DD692 Class Destroyers, 1943 (175 ships)
DE(TEP) Class Picket Ships, 1943 (7 ships)
CVE105 Class Aircraft Carrier Escorts, 1943 (19 ships)
LST542 Class Landing Ship Tank, 1943 (611 ships)
AGP4 Class Motor Torpedo Boat Tenders, 1943 (2 ships)
ARB1 Class Battle Damage Repair Ships, 1943 (3 ships)
LSM Class Landing Ships, 1943 (554 ships)
AS22—U.S.S. *Euryale*—Submarine Tender, 1943 (1 ship)
YMS135 Class Motor Minesweepers, 1943 (347 ships)
ARS(T)1 Class Tenders for Salvage Craft, 1943 (3 ships)
ARL1 Class Landing Craft Repair Vessels, 1943 (3 ships)
APB45 Class Auxiliary Barrack Ship, Self Propelled, 1944 (4 ships)
ARV(E) and (A) Class Aircraft Repair Vessels, 1944 (4 ships)
LSD22—U.S.S. *Fort Marion*—Landing Ship Dock, 1944 (1 ship)
CL119 Class Light Cruisers, 1944 (3 ships)
LSM Class Rocket Ships, 1944 (60 ships)
AG88 Class Ice Breakers, 1944 (2 ships)
AD26 Class Destroyer Tenders, 1944 (7 ships)
LST1153 Class Landing Ship Tank, 1944 (2 ships)

At the same time, the firm continued design services and procured much of the equipment for the multiple destroyer building programs based on the following designs for which working plans were started before our entry into the war:

DD445 Class	175 ships built in 11 shipyards
DD453 Class	48 ships built in 7 shipyards
DD692 Class	175 ships built in 8 shipyards

The key position occupied by the Gibbs organization in large government shipbuilding programs naturally invited several investigations by congressional committees. These committees, sometimes hostile at the outset, invariably ended with praise for the firm's contribution to the national effort. At the end of one such investigation, Gibbs remarked: "Nothing educates a man like being forced to look up the answer to every possible question that can be asked about his business." The care with which he replied to every question raised by the investigators undoubtedly was an important factor in the outcome of these hearings.

The dream of William Francis and Frederic H. Gibbs had been to see a great ocean liner built to their design. Early in their careers this goal had seemed close but it receded during World War I and again when the government decided to recondition the *Leviathan.* The *America,* which was completed just prior to our entry into World War II, was the largest passenger vessel built in the United States up to that time and a fine and popular ship but not a true competitor with the fast superliners, the *Queen Mary, Normandie,* and *Queen Elizabeth.*

At the end of World War II, preliminary studies for a new ocean liner were begun and in 1946 the United States Lines Company commissioned Gibbs & Cox, Inc. to develop contract plans and specifications for the S.S. *United States.*

This superb ship resulted from the synthesis of all the experience gained from passenger ships such as the *Leviathan,* the *Malolo,* the Grace Liners, and the *America* combined with the technical advances made in machinery, structure, materials, and methods developed in work for the U.S. Navy.

The dream of W. F. Gibbs evolved into a practicable concept which developed into an extraordinarily coherent design. His drive and organizing ability enabled him to delve into and guide every detail of the design and construction of this ship, while at the same time he refuted the proposals of those who would have settled for less.

The concept of the ship was simple but difficult to achieve. It had to be the safest ship afloat, with standards of subdivision and fire resistance exceeding those of any other ship. It was to compete with the *Queen Mary* and the *Queen Elizabeth* in luxury, accommodations, and speed, yet be much more economical to operate. At the same time, it was designed to be readily convertible to a troop ship, for which purpose its speed and safety features made it extraordinarily suitable.

That the combination of these many qualities in a single ship was actually accomplished has been amply demonstrated by the remarkable record of performance of the ship over the many years it has been in service.

In April 1949 a contract for construction was awarded to the Newport News Shipbuilding & Dry Dock Company. For a brief time during the building period, the ship was taken over by the U.S. Navy for conversion to a troop ship because of the Korean War. But the construction as a passenger ship was never interrupted; the crisis passed; and the ship was completed on schedule.

On July 3, 1952, the S.S. *United States* sailed from New York on its maiden voyage, with Mr. and Mrs. W. F. Gibbs on board. No previous announcement had been made but the passengers soon realized that it was to be an extraordinary voyage. The ship passed Bishop Rock 3 days, 10 hours, and 40 minutes after leaving Ambrose Light, having made the eastbound passage at an average speed of 35.59 knots. This was almost four knots faster than the previous record of 31.60 knots established by the *Queen Mary* and brought the Blue Riband trophy to the United States for the first time. Another record of 34.51 knots was made on the westbound passage. It was the first time in a hundred years that this country had had the premier ship on the Atlantic.

The ship made triumphal entries into Le Havre and Southampton and was enthusiastically received in both ports. William Francis Gibbs received wide acclaim and many awards for his achievement. But the published comment that he most appreciated was a succinct statement in the charivari column of the British magazine *Punch:* "After the loud and fantastic claims made in advance for the liner *United States* it comes as something of a disappointment to find them all true."

By more than just speed, this ship symbolizes the quality of the design, engineering, and attention to detail that characterizes all of the work done by William Francis Gibbs. Its record for reliability, economy, comfort, and popularity with the traveling public is eloquent testimony to the success of the design.

It was widely reported at the time that the ship had not used nearly its full power during this record-breaking voyage. The secrets of the ship's capabilities were so closely guarded under W. F. Gibbs's ever-watchful eye that very few people, even today, know her maximum power and speed or even what she did on trial.

Soon after the close of World War II, the U.S. Navy recognized that the greatest threat to its supremacy on the seas lay in possible enemy submarine attack. It also realized that its technological advantages had become so well known to other navies that a major effort to achieve another significant technical advance would be necessary if it were to maintain its supremacy. The firm of Gibbs & Cox, Inc. was assigned a major role in this effort, starting with the experimental destroyer, DD828, the U.S.S. *Timmerman,* followed by a series of other designs for destroyers, frigates, escorts, guided missile ships, landing craft, hydrofoils, and others.

The end of the war also brought a demand for many designs for ship conversions and new, modern cargo liners and bulk carriers and the well-known passenger-cargo ships, *Santa Rosa* and *Santa Paula,* which replaced the earlier ships of the same name. The strong interest with which W. F. Gibbs followed every detail of these designs did not prevent him from pursuing many other interests as well.

When the Gibbs brothers were boys in Philadelphia, their father often permitted the family coachman, a former member of the city Fire Department, to drive the boys to fires, taking them from school, if necessary. This developed in W. F. Gibbs a fascination with fire fighting which lasted all of his life. While still in college, he investigated the possibility of designing a fire engine more powerful than those then in service. In 1930 he developed a design for a powerful pumping engine but was frustrated because there was no gasoline or diesel engine available that was sufficiently powerful, light, and compact for his purpose.

Because of his interest in the subject, however, the New York City Fire Department retained him as a consultant and, in 1937, he designed and supervised the construction of the New York City fireboat *Fire Fighter*. Still the most powerful fireboat in the world, the *Fire Fighter* has demonstrated its great value many times at waterfront fires which were unapproachable by other means or even by ordinary fireboats.

The contrast between the performance of the *Fire Fighter* at large fires and the relative ineffectiveness of the weak streams produced by ordinary fire engines remained as a challenge. New and more powerful diesel engines were developed after the war. In 1962, together with the Mack Truck Company, Gibbs developed for New York City a Super Fire Engine Pumper and Tender with the capability of throwing a powerful stream of water into fires which could not be reached by any other equipment. This was not a single vehicle but a veritable task force consisting of the pumping engine and several hose-carrying tenders. It is easily the most powerful weapon against large fires in the world today.

The office of the president of Gibbs & Cox, Inc. was famed for its simplicity and austere appearance. For many years, Gibbs had no desk but perched on a stool at a drawing board. These he retained even after he moved to a chair and a simple table. He worked surrounded by activity and several secretaries were constantly kept busy supplying him with information and transmitting his numerous instructions and messages.

Devotion to ships, fire engines, and business appeared to many to be an all-consuming passion which took W. F. Gibbs to his office for long hours seven days a week. He was usually the first to arrive and often the last to leave.

Francis Gibbs, as he was widely known by family and friends, had many other interests as well. He frequently attended the theater, symphony, and opera and had many friends in the artistic world, for he appreciated quality and superb performance in any field of endeavor. He maintained a box at the Yankee Stadium for many years. He was a vestryman and active supporter of St. Thomas's Church on Fifth Avenue.

A keen sense of humor and a delight in surprising his listeners made him a most effective extemporaneous speaker before an audience of any size. He believed strongly that a sense of humor is vital in any field of endeavor.

Great engineering projects require the coordinated efforts of many people. William Francis Gibbs was a natural leader, who inspired great loyalty in his staff and confidence and cooperation in those with whom he did business. His extraordinary enthusiasm for his work set the example which drove all who worked with him to greater efforts to maintain the high standards of performance that he demanded. He took a close personal interest in his employees and quietly, often anonymously, assisted those in need.

Active almost to the end, William Francis Gibbs died on September 6, 1967, at the age of eighty-one. Following services at the Madison Avenue Presbyterian Church in New York, interment was at the First Presbyterian Church Cemetery, Princeton, New Jersey. He is survived by his wife, Vera; his brother, Frederic; his sons, Francis and Christopher; a stepson, Adrian Larkin; and two grandchildren.

At a meeting of the Council of the National Academy of Engineering on November 16, 1967, the following resolution was adopted:

MEMORIAL RESOLUTION—WILLIAM FRANCIS GIBBS

RESOLVED, that the following statement be placed in the minutes of the Council as a memorial to William Francis Gibbs:

WHEREAS, with the death of William Francis Gibbs on the 6th of September, 1967, the National Academy of Engineering lost one of its outstanding members—a distinguished naval architect and engineer, and a pioneer in ship design and technology who greatly advanced his profession; and

WHEREAS, he will long be remembered by his friends and associates for his vision, his energy, his penetrating intellect and good judgment, his strong sense of public service and responsibility, and his generosity: Therefore be it

RESOLVED, that the National Academy of Engineering record its great sorrow on the passing of William Francis Gibbs.

EDUCATION, HONORS, AND DISTINCTIONS

EDUCATION

Harvard University, 1906-1910
Columbia University Law School, 1911-1913, LL.B.
Also, graduate work in economics, Columbia University, 1911-1913, M.S.

HONORARY DEGREES

Doctor of Engineering, Stevens Institute of Technology, 1938
Doctor of Science, Harvard University, 1947
Doctor of Engineering, New York University, 1955
Doctor of Science, Bowdoin College, 1955

HONORS

1919 War Department Certificate in Recognition of Services, World War I
1943 American Design Award
1946 David W. Taylor Gold Medal (Society of Naval Architects and Marine Engineers)
1946 Phi Beta Kappa (Honorary), Harvard Chapter
1946 Trustee, Thomas Alva Edison Foundation, Inc.
1947 Presidential Certificate of Merit, World War II
1951 Holland Society of New York Distinguished Service Gold Medal
1953 National Transportation Award for 1952
1953 Trustee, United Seamen's Service
1953 Franklin Institute Gold Medal
1955 First Recipient of the Elmer A. Sperry Award
1959 Michael Pupin Medal, Columbia Engineering Alumni Association
1960 Allied Professions Medal, American Institute of Architects
1961 Virginia Museum of Fine Arts Award
1962 William S. Newell Memorial Award, United Seamen's Service
1964 Military Order of the World Wars, New York Chapter, Certificate of Recognition of Services
1964 Elected Benjamin Franklin Fellow of Royal Society of Arts

1964 Military Order of the World Wars Citation for Distinguished and Exceptional Services
1965 Navy League of the United States, New York Chapter, Scroll of Honor in recognition of Services

MEMBERSHIPS

American Bureau of Shipping: Technical Committee
American Philosophical Society
American Society of Mechanical Engineers: Honorary Member (Fellow)
American Society of Naval Engineers
British Nuclear Energy Society
Edison Foundation Trustee
Institute of Aerospace Sciences
Military Order of the World Wars
National Academy of Engineering (1965)
National Academy of Sciences (1949)
Navy League of the United States
Newcomen Society
New York Academy of Sciences
New York Bar Association
North East Coast Institution of Engineers & Shipbuilders
Phi Beta Kappa Association
Royal Institution of Naval Architects
Royal Society of Arts
Society of Naval Architects and Marine Engineers
United Seamen's Service

CLUBS

Broad Street
Century
Essex County
India House
James River Country
New York Yacht
Piping Rock
Propeller
University

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"Technological Revolutionist," *Time,* September 28, 1942
"The Mysterious Mr. Gibbs," *Saturday Evening Post,* January 20, 27, and February 3, 1945
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"Mr. Gibbs' Baby," *New Yorker,* November 16, 1957
"Profiles: The Best I Know How," *New Yorker,* June 6, 1964

MELVILLE JEAN HERSKOVITS

September 10, 1895–February 26, 1963

BY JOSEPH H. GREENBERG

MELVILLE J. HERSKOVITS was, at the time of his death, the acknowledged dean of African studies in the United States as well as a major figure in the world of anthropology. His achievements were impressive, whether measured in terms of field work, scholarly publications, organizational activities, or the training of students. His outstanding personal characteristics were a well-nigh boundless energy and enthusiasm for every aspect of his multifarious activities and a wide range of scientific and humanistic interests. His wife, Frances, a professional anthropologist in her own right, was his lifelong collaborator and co-worker whose contributions are not to be measured solely from the list of works which she co-authored with him.

Melville Jean Herskovits was born in Bellefontaine, Ohio, on September 10, 1895; he lived there until the age of ten. He subsequently lived in El Paso, Texas, and Erie, Pennsylvania, where he graduated from Erie High School in 1912. In 1915 he entered the University of Cincinnati and the Hebrew Union College, the latter for theological studies. In World War I he served in the Medical Corps and on his return he entered and graduated from the University of Chicago as a history major. Another undergraduate subject of interest was biology, the training in which was to stand him in good stead in his physical anthropological researches.

At the time of his university studies anthropology hardly existed as an undergraduate area of specialization. Hence, like many others who became professionals, he first became seriously interested in anthropology as a career in the course of his graduate studies. It was then that he came under the influence of Alexander A. Goldenweiser of the New School for Social Research and of Franz Boas at Columbia University in New York. In anthropology as conceived by these men Herskovits found the methodology for the study of human history and behavior and a body of theoretical knowledge which supplied what he had felt was lacking in the other disciplines in which he had worked. In particular Franz Boas, indisputably the leading figure in American anthropology during this period and long afterward, helped to shape his outlook so that he always considered himself as one who continued the Boasian tradition. He was later to write a biography of Boas and to be his staunch defender when, after Boas' death, a certain reaction set in against his theoretical approach and his personal contribution was deprecated. Another intellectual influence which did much in shaping Herskovits' views on the economics of nontechnological societies was that of Thorstein Veblen, with whom he had frequent contacts during the early and mid-1920s, both at the New School for Social Research in New York and later in Washington at the time that he was conducting physical anthropological research and teaching at Howard.

Herskovits received his Ph.D. degree at Columbia in 1923; his thesis, "The Cattle Complex in East Africa," was carried out under Boas' supervision. This study became the starting point of his fundamental work on the classification of African cultures into "culture areas," which appeared in preliminary form in 1924 and more definitively in his well-known paper "The Culture Areas of Africa" (*Africa*, 3:59-77, 1930). Although culture area analysis is today by no means so prominent a theoretical issue as it was at that time, Herskovits' classification is still of value for historical and descriptive analysis, and many a course on African ethnology still employs it as a theoretical framework.

After receiving his doctorate Herskovits became one of the original fellows of the Board of the Biological Sciences of the National Research Council, under whose auspices he carried out a four-year study of variability of the American Negro under race crossing. His major conclusion was that, contrary to what might have been expected in then current genetic theory, variability had not increased and that the American Negro had evolved into a distinct racial type different from the parent stocks, a type, moreover, which was roughly as homogeneous as any other recognized racial group. Another important facet of this study was the attention paid to sociocultural factors of selection. The results are described in Herskovits' book *The American Negro: A Study in Racial Crossing* (New York, 1928). These conclusions and the data on which they were based were likewise presented in more technical form in a monograph, *The Anthropometry of the American Negro* (New York, 1930), which appeared in the series Columbia University Contributions to Anthropology.

In 1924 he married Frances Shapiro. Their only child, a daughter, Jean, became a historian with a specialized interest in West Africa. In 1927 Herskovits joined the faculty of Northwestern University where he founded the Anthropology Department and where he continued throughout his scholarly career.

From Herskovits' study of the physical anthropology of the American Negro there emerged during this period what was to be the major research interest of his career, the systematic comparative study of New World Negro cultures in relation to their source cultures in Africa, particularly West Africa. In

Herskovits' view the variations the New World cultures exhibited in relation to the different situations into which the Negro was brought as a slave constituted a vast comparative laboratory from which might emerge results of general value to the understanding of the interrelationship between culture and physical form as well as the processes of cultural change. Moreover, such studies would shed light on the basic nature of Negro culture.

He hypothesized that the most illuminating method of studying the nature of cultures is by considering conditions in which the culture has maintained itself under stress and strain. In the case of the New World Negro, what was retained, what discarded, and what modified would point the way to a better understanding of what was found in Africa itself as well as make for a better understanding of the process of culture as a whole.

In accordance with this program, he undertook a series of field studies of broad scope. The results of these studies were largely embodied in a series of books, *Rebel Destiny* (1934, with Frances Herskovits), *Suriname Folk-lore* (1936, with Frances Herskovits), based on work in Dutch Guiana, *Life in a Haitian Valley* (1937), and *Trinidad Village* (1946, also with Frances Herskovits).

It was only natural that Herskovits should not be content with descriptions of African cultures in the ethnographic literature but should wish to investigate at firsthand the African background of the New World Negro. He therefore undertook during the mid-1930s a field study of the West African kingdom of Dahomey, the direct source of many African elements of New World Negro culture, particularly in Haiti, but also in Cuba, in Trinidad, in Brazil, and in Louisiana. This investigation resulted in the monograph *Dahomey: An Ancient West African Kingdom* (2 vols., 1938), which by reason of its breadth and detail of documentation still retains its place as a classic of

African ethnography. In addition to this monograph, Herskovits produced two other major contributions to the study of Dahomey in collaboration with his wife, *An Outline of Dahomaen Religious Belief* (1933) and, much later, *Dahomean Narrative: A Cross-Cultural Analysis* (1958).

One of the important by-products of the foregoing research was the light that it shed on the African sources of New World Negro slaves; these sources differed significantly in different New World areas and were reflected in the varying patterns of African survivals. It is not too much to say that with this pioneering series of researches Herskovits virtually founded Afroamerican studies as a scientific field in its own right. Outside of its purely scientific value, this work had important implications for the way in which the Negro in the New World conceived himself and was conceived of by others, at a time when the prevailing view of both Negro and non-Negro was that the Negro of the Americas was, so to speak, a man without a past. Though today the idea of a Negro past is generally accepted, at the time the demonstration of the cultural continuity between Africa and America was profoundly new, and disturbing of generally held stereotypes. The work which more than any other put in a broader and more generally accessible framework the results of these researches was *The Myth of the Negro Past* (1942). Its very title embodies the main thrust of the work, that the myth regarding the Negro past is that he has no past.

It was characteristic of Herskovits that while his work was thus grounded in the concrete yet broadly conceived historical reality of the Negro in Africa and America, he never lost sight of the ultimate concern of such work, namely, its relevance to the problems of the science of anthropology as a whole. Since the study of the Negro involved, as it did, the wholesale transplantation of populations from one hemisphere to another by an external agent and a wide variety of patterns of interaction among Negro, Indian, and Caucasian in the New World, it could not be understood in terms of descriptions of aboriginal life abstracted from the effects of more recent outside influences, as was the practice of classical ethnographic description. This was coming to be realized more and more by anthropologists during the 1930s. The study of the contact of cultures as a scientific field in its own right was emerging and came to be called acculturation in the United States. It was not surprising, then, that Herskovits was appointed by the Social Science Research Council in 1936 as one of a committee of three distinguished anthropologists numbering, in addition to Herskovits, Ralph Linton and Robert Redfield, for the purpose of drafting a memorandum which would define the scope and content of acculturation as a topic of scientific investigation. The resulting document was widely disseminated and exercised a considerable influence on the development of the area. To this field, Herskovits made a further substantial contribution in his book *Acculturation: The Study of Culture Contact* (New York, 1938).

To the impressive list of Herskovits' achievements prior to the outbreak of World War II there should be added at least one further major effort, his pioneer work *The Economic Life of Primitive Peoples* (New York, 1940). This was the first full-fledged work by an anthropologist devoted to the economics of nonliterate people, as distinct from their technology, a traditional field of anthropological investigation. In this volume, Herskovits sought to build a bridge between the discipline of economics and anthropology. On the one hand, he sought to broaden the scope of economics, which in practice confined its attention to industrial societies, by providing a body of empirical data regarding the economic life of tribal societies to replace the hypothetical constructs in vogue among economists. On the other hand, he sought to stimulate the study of economic institutions in the proper sense among anthropologists, who up to then had mainly concerned themselves with the details of technology and of custom and ceremonial surrounding economic activities. Thus he sought to advance economics as the general science of human economic behavior by stimulating the collection by anthropologists of quantitative and other data relevant to the theoretic concerns of the economist.

In this area there was indeed a considerable change for which Herskovits could claim much credit. Thus he was able to state in his introduction to the extensively revised edition of his earlier work, which appeared in 1952 under the title *Economic Anthropology:*

"The neglect by earlier anthropologists of the economic aspects of the cultures they studied no longer exists. Under present conventions of field-work no anthropologist of competence takes as synonymous the technology of a people with their economics, or considers it sufficient if he only studies the canon of ownership, where problems of differentials in wealth and position are his concern."

During World War II, Herskovits served as Chief Consultant for African Affairs of the Board of Economic Warfare. Africa, of course, played an important strategic role in the conduct of the war. It became painfully evident that American scholarly neglect of Africa, as of other non-Western areas, was a severe handicap to the national interest.

It was one of the postwar outcomes of this situation that in 1947 Herskovits was requested by the Carnegie Corporation to institute interdisciplinary research and teaching of African topics at Northwestern University. This resulted in 1948 in the formation of the Program of African Studies, the first of its kind in the United States. Herskovits held the position of director of the program for the remainder of his career.

The Northwestern program was noteworthy for the number of professional Africanists it produced, particularly in Herskovits' own field of anthropology. Some notion of the scope of the program and the scholarly results it produced can be obtained from a perusal of *Continuity and Change in African Cultures.* This volume, which appeared in 1959, was jointly edited by Herskovits and William Bascom, the latter a pupil of Herskovits and in his own right a notable folklorist and museologist. It consisted exclusively of contributions by scholars who had received training at Northwestern University. The general theme of the book, as indicated by the title, was closely connected with culture change, which has already been indicated as a central theoretic concern of Herskovits. The title reflects Herskovits' contention that stability is no more to be taken for granted than change. The basic continuities of culture require explanations also, for, as stated in the joint preface in reference to the political changes in Africa leading to the independence of many African states, ". . . these surprisingly rapid political changes have not destroyed the continuity of African cultures."

In the period following World War II, the rapid political and economic changes and the factor of American involvement in technical assistance programs produced a growing interest in the study of change in non-Western areas. What distinguished Herskovits' approach as reflected in this book is the unwillingness to confine or isolate the study of stability and change to socioeconomic and political aspects. Thus there are one or more papers in *Continuity and Change in African Cultures* devoted to language, art, music, indigenous social organization, economics, and religion.

With the constantly expanding role of Africa in world affairs in the postwar years, those engaged in the formulation of American policy in Africa turned to Herskovits as the outstanding expert on Africa. In 1959 he prepared *United States Foreign Policy: Africa* for the Committee on Foreign Relations of the United States Senate.

Also in 1959 Herskovits became a member of the National Academy of Sciences.

In addition to the direction of the African program at Northwestern and the chairmanship of the Anthropology Department, which he relinquished in the late 1950s, Herskovits devoted much of his energy to activities which were of value to the anthropological profession as a whole, notably as editor of the third edition of the *International Directory of Anthropologists* (1950) and as editor (1949-1953) of the *American Anthropologist,* the journal of the American Anthropological Association.

In spite of these heavy commitments in the postwar years, Herskovits continued to be a highly productive scholar. The sweeping changes which anthropology had undergone during and after the war, and its rapid expansion both as an undergraduate subject and in its traditional graduate school role, resulted in a rising demand for general anthropology texts, of which there were very few at the time. Herskovits' *Man and His Works* (New York, 1948) helped in large measure to fill this *desideratum.* As might have been expected from Herskovits, the work was notable for its breadth of topical coverage and its command of an enormous literature. It was widely adopted as a textbook in introductory courses. In view of its considerable length, however (673 pages), it was reissued in an abridged form with the title *Cultural Anthropology* (New York, 1955).

There has been occasion earlier to mention two of Herskovits' other books which appeared during the period 1950-1960, his biography of Franz Boas (1953) and his extensive work on Dahomean folklore which appeared under the title *Dahomean Narrative: A Cross-Cultural Analysis* in 1958 and was written in collaboration with his wife, Frances.

Herskovits' last major work was one which sought to analyze and put in historical perspective the vast changes undergone by the African continent and to which he had devoted so much of his life and energy. *The Human Factor in Changing Africa* is a veritable tour de force in summarizing and helping to make comprehensible the vast panorama of change over the entire continent.

Another important aspect of Herskovits' work during this period was his collaboration with two psychologists, Marshall Segall and Donald T. Campbell, on cultural influences in perception. The results of these researches were published posthumously in 1966.

It was natural that Herskovits, who had always been a staunch proponent of the value and dignity of African culture, as indeed of all human cultures, should greet with enthusiasm the accession of independence of African states which had been under colonial rule. It was peculiarly fitting that his last public activity was the major role he took in the organization of the First International Congress of Africanists, held in Ghana in December 1962. Traditionally, Africanists had met as a subsection of the International Congress of Orientalists. It was at the Moscow Congress of this latter association that Herskovits, together with I. I. Potekhin of the Academy of Sciences of the USSR, took the decisive step of organizing African Studies internationally, independent of Oriental Studies. The Congress at Accra, at which Herskovits delivered a notable address summarizing the history of Africanist studies, was remarkable also in that Africans themselves were prominently involved in the work of the Congress. It was hardly two months after this Congress that Herskovits passed away in Evanston. As evident from the foregoing sketch, which summarizes but a portion of his numberless activities, Herskovits was a man of truly extraordinary breadth and energy. His impact on the fields in which he was active is still with us. None who came into contact with his writings could fail to profit from his inquiring mind and his outstanding gift for scientific synthesis. Those who had the privilege of knowing him personally will never forget his warm and vibrant personality. In particular his students, among whom the present writer is proud to number himself, can attest that there was no limit to his loyalty and willingness to provide guidance and stimulation.

HONORS AND DISTINCTIONS

DEGREES AND PROFESSIONAL POSITIONS

Ph.B., University of Chicago, 1920
A. M., Columbia University, 1921
Ph.D., Columbia University, 1923
Lecturer in Anthropology, Columbia University, 1924-1927; Howard University, 1925
Assistant Professor of Anthropology, Northwestern University, 1927-1930
Associate Professor of Anthropology, Northwestern University, 1931-1935
Professor of Anthropology, Northwestern University, 1935-
Professor of African Affairs, Northwestern University, 1960-

PROFESSIONAL SOCIETIES AND OTHER MEMBERSHIPS

Fellow in Anthropology, Board of Biological Sciences, NRC, 1923-1926
Chairman, Committee on African Anthropology, and Chairman, Committee on International Relations in Anthropology of the Division of Anthropology and Psychology, NRC, 1942-1950
Chairman, Committee on Negro Studies, American Council of Learned Societies, 1939-1950
Fellow, National Association of Sciences, AAAS (Vice President, 1934)
American Anthropological Association (President, Central Section, 1939; Executive Board, 1947)
Editor, *American Anthropologist,* 1949-1952
African Studies Association (President, 1957-1958)
Member, American Association of Physical Anthropology
American Folklore Society (President, 1945)
Société des Africanistes de Paris
International African Institute (Executive Council)

HONORS

Guggenheim Memorial Fellow, 1937-1938
Officer, Order of Honor and Merit, Haiti
Officer, Order of Orange-Nassau, The Netherlands
Honorary Fellow, Royal Netherlands Geographical Association
Honorary Fellow, Royal Anthropological Institute
President's Fellow, Northwestern University, 1960
Viking Fund Medalist, 1953

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KEY TO ABBREVIATIONS

Am. Anthropol. = American Anthropologist
Am. J. Phys. Anthropol. = American Journal of Physical Anthropology
Am. J. Sociol. = American Journal of Sociology
Am. Soc. Rev. = American Sociological Review
Human Biol. = Human Biology
Internat. Soc. Sci. Bull. = International Social Science Bulletin
J. Am. Folklore = Journal of American Folklore
J. Negro Hist. = Journal of Negro History
J. Roy. Anthropol. Inst. = Journal of the Royal Anthropological Institute
J. Social Forces = Journal of Social Forces (later changed to Social Forces)
Revta. Bras. = Revista do Brasil
S. W. J. Anthropol. = Southwestern Journal of Anthropology

1923

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1924

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1925

Preliminary observations in a study of Negro-White crossing. Opportunity, 3:69-74.

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Some aspects of the anthropology of the American Negro. Journal of the Washington Academy of Sciences, 15:225-26. (A)

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A further discussion of the variability of family strains in the Negro-White population of New York City. Journal of the American Statistical Association, 20:380-89.

Social pattern: a methodological study. Social Forces, 4:57-69.

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R. H. Kent.

ROBERT HARRINGTON KENT

July 1, 1886–February 3, 1961

BY LESLIE E. SIMON, FRANK E. GRUBBS,
AND SERGE J. ZAROODNY

ROBERT HARRINGTON KENT was born in Meriden, Connecticut, on July 1, 1886. He attended Harvard University, obtaining his Bachelor of Arts degree in 1910 and his Master of Arts degree in 1916; in 1953 his alma mater conferred upon him the honorary degree of Doctor of Science. After graduation he became an assistant instructor in physics and a part-time instructor in mathematics at Harvard. He was an instructor in electrical engineering at the University of Pennsylvania from 1916 to 1917.

In 1917 Kent entered the Army and was commissioned a First Lieutenant in the Ordnance Department. He was assigned to the Office of the Chief of Ordnance, Washington, D.C., and in June 1918 was ordered to Tours, France, on the staff of the Chief Ordnance Officer, American Expeditionary Forces. He was in charge of ballistic work and was responsible for the preparattion of firing tables for the use of American artillery.

He resigned from the Army and entered upon duties as a civilian in July 1919 in the Office of the Chief of Ordnance, Washington, D.C. In 1922 he was transferred to the Aberdeen Proving Ground, Maryland. He worked continuously in the field of the relatively rare and complex science of ballistics; he was probably our country's greatest expert in interior, exterior, and terminal ballistics, and was exceptionally adept in all fields of science and engineering that support ballistics. Moreover, Kent was a highly competent, self-taught statistician. Kent became Associate Director of the Ballistic Research Laboratory when it was organized in 1938 and held the same title after the organization was expanded a few years later to become the Ballistic Research Laboratories (BRL). In the early years he was associated with (then) Colonel H. H. Zornig and (then) Major-Colonel Leslie E. Simon, who were the first and second directors of the BRL.

Kent was Chairman of the Explosives and Armament Panel, United States Air Force Scientific Advisory Board, during World War II. He was also a member of the Adivsory Board, Naval Ordnance Test Station, Inyokern, China Lake, California.

He was a fellow of the American Association for the Advancement of Science and of the American Physical Society. He was a member of the Institute of Mathematical Statistics, the Institute of Aeronautical Sciences, the National Academy of Sciences (1951), and Phi Beta Kappa.

He was decorated with the Presidential Medal for Merit in 1946, the Potts Medal of the Franklin Institute in 1947, and the Campbell Medal of the (Army) Ordnance Association in 1955.

In the course of his two-score years in ballistics he made many original contributions to ballistics and also other sciences, as attested in his numerous published papers and reports. He was, almost surely, the country's only "complete" ballistician.

However, Kent's contributionns to his fellow men, his leadership, his warm friendship, and his generous understanding are no less important than his scientific work. He was a master of the art of gathering together people of competence and infusing them with the scientific method and causing them to produce new and original work. He was more than a mere catalyst because, unlike a catalyst, he almost invariably entered into the reaction that he stimulated and made important contributions to the work that his guidance and suggestions had initiated.

KENT AS A STUDENT AND EDUCATOR

Kent's entire life was an intimate mixture of being a keen and intelligent student, doing important and absorbing work, and teaching in a competent and kindly way those people who chose to be his students and who needed his help. Even his own formal education was interrupted by periods of teaching. His family first sent him to Columbia College to become a preacher; somewhat against the wishes of his family, he managed to transfer to Harvard and was graduated a Phi Beta Kappa, in 1910. In the next six years he was both student and teacher. He taught physics and mathematics at Harvard while leisurely studying for his Master of Arts degree, which he obtained in 1916. He also passed his examinations for his doctor's degree, but at this point he had a fundamental difference with Harvard. Kent wished to write his doctoral thesis on the determination of the laws of intermolecular repulsion from the experimentally determined virial coefficients in Van de Waal's equation. Harvard insisted that physics was an experimental science and declined to award a doctorate for theoretical work only. As a matter of principle, Kent declined to undertake the experimental work. Thus it was that a man who was destined to blend beautifully the keen reasoning of the theorist with the objectivity of the experimentalist sacrificed his doctorate as a protest against the rigid rules of Harvard. How strongly Kent felt about his principle is illustrated by the fact that, until he received his honorary doctorate from Harvard in 1953, he would never permit his being addressed as "Doctor" Kent. The unknowing person who so addressed him was immediately greeted with a friendly smile and the good-humored rejoinder, "Thank you for the doctorate."

In the long run Kent won. Eventually, Harvard did grant doctorates for theoretical work, and Lennard Jones gained very wide fame for work similar to that which Kent wished to do as his doctoral dissertation. In 1953 Kent, the world-renowned military scientist (both theoretical and experimental), was awarded an honorary degree of Doctor of Science. In awarding the degree, Harvard's President Conant observed with kindly good humor: "In a world of violence, he measures explosive forces and predicts where the modern arrows shot into the air will land." What more could one say of a man whose lifework exemplified so keenly the scientific method of hypothesis, experiment, and judgment of hypothesis?

In the interval between the award of his M.A. degree in 1916 and World War I, Kent spent a year at the University of Pennsylvania as an instructor in electrical engineering. One may well suspect that he wished a better background in electricity than he had gained from his physics and thus chose, like many academic people, to teach a course in a subject as an avenue to self-education. In any event, he learned well because he later did some illustrious electrical work in connection with instrumentation for ballistic experimentation; see, for example, "The Propagation of Electric Currents in Terminated Lines: Solutions of the Telegraphic Equation," *Physical Review,* Vol. 55, No. 8 (1939).

Kent never accepted a formal academic post after his year at the University of Pennsylvania, but his most important work as an educator still lay before him. He had a warm, generous personality, he had a depth of human understanding, he loved people, and he was ever happy to assist any intelligent and conscientious student. He gave lectures at the Ordnance School, Aberdeen Proving Ground, and the Navy Post Graduate School, Annapolis, Maryland; wrote prolifically for ordnance textbooks for the U.S. Military Academy, West Point, New York; was a leader in the colloquia at the Ballistic Research Laboratories; and was ever an aid to the Ballistic Institute (for graduate study) of the Laboratories. However, his most important educational work was a personal affair. The number of young Army officers that he assisted in scientific matters, guided, and stimulated is legion. When young officers who had been stationed with Kent wrote him about their problems, he never failed to give them personal assistance, references in the literature, and encouragement. He trained many officers for special assignments; however, his greatest continuing work consisted of the advice, council, and guidance he dispensed to his own staff and to many young men and women in the Ballistic Research Laboratories. He once gave a very interesting talk entitled "Two-Score Years in Ordnance." It might well have been entitled "Two-Score Years in Teaching."

KENT AS AN ARMY OFFICER

It is probable that Kent, himself, gave the best possible account of his brief (approximately fifteen months) career as an Army officer. The following are excerpts from his talk "Two-Score Years in Ordnance":

"When America got into World War I in 1917, I decided that I should do something for my country. I was, at that time, an instructor of electrical engineering at the University of Pennsylvania. When the term ended, I went to a recruiting officer of the Signal Corps to volunteer my services in the Aviation Branch of the Corps because I thought that would be the heroic thing to do. I was tremendously relieved when the officer told me that I was four months too old to be an aviator. Now at that time, of course, the landing speed of airplanes was 30 or 40 miles an hour. I think that the aviators of the present day are much braver than I would have been if I had entered the Signal Corps, because the landing speeds now are 80 or 100 miles an hour.

"From Philadelphia, I went to the Officers' Training Camp at Fort Oglethorpe, Georgia, a short distance from Nashville,* Tennessee, I believe. When they asked me what my choice would be—Infantry, Field Artillery, Coast Artillery—I thought that, since I had received some training in mathematics and physics, I should enter the Coast Artillery. About September, I think it was, we went down to Fort Monroe, Virginia, where my training for the Coast Artillery began.

"Now at Fort Monroe, at the close of the camp, there was an Ordnance officer who was recruiting for the Ordnance Department and he offered me a commission of First Lieutenant in Ordnance. I thought I didn't want to be an 'embusqué' [hiding behind the bushes], you know, and so I thought I would be brave and enter the Coast Artillery Corps in spite of the Ordnance officer's offer. However, when I told my company commander that I had been offered a commission in Ordnance as well as in Coast Artillery, he said, "Kent, I advise you to take Ordnance because, in my opinion, you're not a very military person." I must say that I stammered then worse than I do now. So that was the reason why I went into Ordnance.

"I arrived at the Ordnance Office in Washington about December 1, where I met Lieutenant James (Jimmy) Alexander. He was my co-ballistician in the Office of the Chief of Ordnance. About March 1918, Major F. R. Moulton came to take charge of Lieutenant Alexander and me in the Ballistic Section, Ammunition Branch, Gun and Carriage Division, Office of the Chief of Ordnance. Major Moulton had been Professor of Astronomy at the University of Chicago. He had a considerable reputation as the co-author of the Chamberlin-Moulton planetesimal theory concerning the origin of the solar system. He was also well known for his computation of the orbits of the planets, for which he had developed methods of numerical integration.

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* Chattanooga, Tennessee, actually.

"Shortly after Major Moulton arrived in Washington, Captain F. Wheeler Loomis came back from France and requested Moulton to undertake the computation of antiaircraft trajectories according to methods that had been developed by R. H. Fowler and others in Great Britain. Moulton replied that he was unwilling to undertake such computations. He intended to get out a better method than Fowler's in a short time. As a result of this, Captain Loomis went down to Aberdeen Proving Ground, where, in association with Major Oswald Veblen, the calculations were begun on the trajectories of AA [antiaircraft] guns.

"Major Moulton eventually developed his well-known method of numerical integration of trajectories of shell. This method and also various other matters are explained in his book *New Methods in Exterior Ballistics,* published in 1926. The book included a discussion of the motion of a spinning shell about its center of gravity. In this theory, Moulton gave the damping in pitch as the only means of damping of the initial yaw. He did not include the lift or the cross-wind force. The result was that he did not succeed in getting the initial yaw damped out, although he thought he did under certain circumstances.

"His method of the numerical integration of the trajectory of the shell was, however, a very important contribution to exterior ballistic theory. It was the method that was used in the computation of Volumes I, II, and III of the Tables for Exterior Ballistics.

"Lieutenant Alexander and I made firing tables for some ammunition for the 3.2-inch gun. In the process of making these firing tables, especially in the preparation of the tables for the wind effects, we discovered that an easy method of allowing for these effects was to use a platform which moved with the wind. Our ego was somewhat deflated a week later when we found that this was a well-known method in exterior ballistics.

"Incidentally, the startling news that the Germans had fired on Paris from a distance of 75 miles arrived at our office on 24 March. Lieutenant Alexander, by approximate methods, computed that a muzzle velocity of about 5,000 ft/sec should be sufficient to give a heavy 8-inch shell such a range.

"The Ordnance Department had made some American shrapnel for the French 75mm gun. I made some firing tables for these shrapnel in the office in the F street Building—no, it wasn't, it was probably in the Ford Building. We moved all around in those days. We would stay a month in one place and then we would stay a month in another place. We did a lot of moving. Anyway, I, with the aid of secretaries who were available there in the office, constructed firing tables for the American shrapnel in the 75mm gun. I had the firing tables sent to the government printer and had read the first proofs of the firing tables when I went overseas.

"About the end of June or the middle of July 1918, I entered the Office of the Chief of Ordnance Officer of the American Expeditionary Forces in Tours, France. Anyway, I waited and waited. Finally Colonel Steese, my immediate superior, said that they needed some firing tables for American shrapnel to use at the Field Artillery Center at Is-Sur-Tille. So I took the photostat copy of the table that I had brought with me to the French printer in Tours. In two weeks I had a couple thousand copies of the firing tables and I sent them to the Field Artillery Training Center.

"Ordnance had a lot of new ammunition that was going to be used, for which they needed firing tables. Some of it was for the 4.7-inch Gun Model 1906, which was one of the early guns with which we entered World War I. Then there was the

8-inch Howitzer and a 6-inch Gun Model 1905. These were American guns and we needed firing tables for all of them.

"I didn't know much then; well, perhaps I have learned something since. I just didn't realize that I could have written to my friend Major Veblen, who had charge of the firing tables and range firing at Aberdeen Proving Ground, and he would have been just delighted to have made the firing tables. So I did the best I could.

"Incidentally, there were some 6-inch shell that were sent over to be tested at a French camp called Mailly, 200 miles or so from Tours and about 100 miles from Paris. There was a Coast Artillery Colonel by the name of Rhodes who suggested a modification for the shell. These shell had a very interesting characteristic. They had a range that was practically independent of the elevation. If you pointed the gun this way [indicating elevation of 20°] they went so far; if you pointed the gun this way [indicating elevation of 45°] they had just about the same range!

"There was a time in April 1919 when they wanted me to take back a company of troops to the United States. Since I really had not been associated with the troops at all, I knew I would do a very poor job of taking a company of troops back. Fortunately they wanted some new firing tables so they ordered me back to Tours. Finally I got back to the United States on the Fourth of July 1919."

In addition to his rather tedious duties with respect to firing tables, Kent served the Chief Ordnance Officer as his representative at various tests and demonstrations of American and French weapons. These latter duties afforded him an opportunity for association with some brilliant young British scientists who had entered into war work. Among these were Captain Fowler and Captain Crow, later to become Sir Ralph Fowler and Sir Alwyn Crow.

In the course of his associations Kent gathered many ideas which he later exploited for the American Army. His ideas for the 3.3-inch aerodynamics range were obtained from Captain Fowler and the American Captain Alexander. He became familiar with the British solenoid chronograph on a visit to Shoeburyness in 1918.

The ideas, or the stimuli for ideas, which Kent acquired abroad were developed by him so effectively in subsequent years that our foreign friends learned to look to Kent's work to find answers to their problems.

Kent returned to this country as a captain in 1919 and was offered a permanent commission. He might have been a general years ago and retired from the Army, if the uniform had been different. However, Kent simply could not stand the restraint and heat of puttees around his legs, so he resigned his temporary commission and entered civil service with a view to using the fruits of science and engineering to improve our weapon systems.

KENT AS A CAREER BALLISTICIAN

When Kent began his career as a civilian ballistician, he worked first in the Office of the Chief of Ordnance and originated the famous 3.3-inch gun program which was to give the first scientific bases for the drag functions of artillery shell and for the design of projectiles of modern shape.

However, with the limited appropriations obtained as a result of the depression of 1921, the Ordnance Corps felt it could not maintain him in Washington. Also, Kent was impatient with the remote control of the experimental phases of his work at the Aberdeen Proving Ground, so he was transferred to Aberdeen in 1922.

During the sixteen years from 1922 to 1938, Kent was the outstanding carrier of the torch of science in the Army. Some men achieve a degree of greatness such as Kent had already achieved by 1922, and then become a landmark rather than a beacon. Kent was a beacon, but not a landmark, because his scientific modesty, his warm human qualities, and his scorn of all pomp and ceremony precluded his ever allowing any one to look up to him as being the dignified incumbent of some authoritative official position. His scientific product was prodigious, and his applied research drew on almost all fields of science and engineering.

In 1920 he measured the loss of spin of projectiles. Because he did not know much about chronographs at that time, he had a misplaced confidence in the accuracy of the Boulengé chronograph. It was not until he talked to Fowler in 1938, who told him that the British suspected from their fuse that projectiles did lose spin, that he reworked the results of 1920. He found that there was indeed a substantial loss of spin. His results have been confirmed by modern experiments.

Stimulated by Fowler's work on yawing shell in Great Britain, he investigated the force system of a yawing shell. Each area of investigation in one field led him naturally to investigations in others. For example, having studied the flight of the shell, he then had to study the effect of the launching conditions, such as the clearance between the bourrelet and the bore. It was appropriate that he was placed in charge of the interior ballistic firings for the 240-mm howitzer. For these, he perfected the piezoelectric gauge.

An enumeration of all his work having to do with the fundamental principles involved in projectiles, bores of cannon, heat transfer, ignition of propellants, bombs, armor plate, fuses, recoil mechanisms, and other types of material and components would be tedious. There is hardly a nook or cranny in the whole field of modern weapons that escaped Kent's scrutiny or failed to benefit from his searching and scholarly study and his brilliant rationalization.

His achievements are especially marked because they were done not in the rich atmosphere of stimulation by daily association with many distinguished colleagues but almost in an atmosphere of seclusion, in the sense that only Kent and less than a dozen others of real professional training were working together. These sixteen years of effort contributed enormously to our readiness in Army weapons (including bomb design and bombing tables) when we were faced with World War II and have affected favorably practically every weapons system now extant.

One should not infer from the above remarks on the uniqueness of Kent's leadership that he worked under conditions of adversity. Invariably, each succeeding Commander of the Aberdeen Proving Ground learned to know Kent, and respected him and supported him. In addition, he received the warm support of officers who had served with him, officers whom he had instructed in courses at the proving ground, and officers and civilians throughout the world who took an intelligent interest in his work and with whom he kept up a voluminous correspondence.

The year 1938 was a turning point in Kent's career because that was the year in which the Ballistic Research Laboratory was organized under Colonel H. H. Zornig as Director and Kent as Associate Director. This was the beginning of a period in which Kent was forced gradually and reluctantly to shift his major emphasis from research work to research administration. Kent's research work continued at full pace for some time because the laboratory underwent no substantial expansion until about the beginning of World War II, when it expanded rather rapidly from about forty employees to five hundred.

One of the most cogent factors in the newly organized Ballistic Research Laboratory was the formation of the Scientific Advisory Committee, composed of eight men highly eminent in their respective fields of science who voluntarily served the laboratory. It is believed that the original idea for such a committee should be attributed to Colonel H. H. Zornig, the first BRL Director; but Kent's counsel in the selection of the members was most valuable, the well-known high caliber of his work contributed to persuading them to serve the government's best quality laboratory, and his warm, genial personality made every meeting a pleasure. The original committee consisted of Harold Urey, I. I. Rabi, Hugh Dryden, Bernard Lewis, A. W. Hull, Henry N. Russel, Theodore von Kármán, and J. von Neumann. Their first meeting was in September 1940; at the time of Kent's death in 1961, two of this illustrious group were still serving.

Kent paid considerable attention to various facilities the BRL acquired over the years. Perhaps he was prouder of the first United States supersonic wind tunnel, installed at BRL and dedicated in 1944, than of any other facility. Dr. Edwin P. Hubble was the first head of the Supersonic Wind Tunnel (or Exterior Ballistics) Laboratory and Kent was its second head when Hubble returned to the Mount Wilson Observatory at the end of World War II.

The Wealth of high-quality personnel obtained for the BRL was due in large measure to the able and influential assistance of Professor Oswald Veblen of the Institute of Advanced Study, Princeton, New Jersey, who, as a major, was in charge of ballistics at the Aberdeen Proving Ground during World War I. However, the able men who joined the BRL had to be oriented in their new type of war work, and Kent proved himself to be as competent a research leader as he had already proved to be a research worker. Kent's method of going about his work engendered an atmosphere of true scientific inquiry and filled his colleagues with enthusiasm, and his guidance led them to swift and fruitful achievements. Kent's powers were multiplied many times by his ability to work through others, and the people who are indebted to Kent are legion. Of course, Kent did not stick to science, to engineering, or even to technical matters. If anything was wrong, he either righted it or demanded a clean-cut accounting of the situation. He had no patience with decisions made without discussion with the people affected—and he was always one of the people affected!

Kent was a highly competent research leader, planner, and administrator of scientific effort. Not so much could be said for him as a manager and business administrator. One of his more businesslike colleagues once remarked, "Bob has a heart as big as a water bucket, and would pay every man the highest salary listed in the civil service table if he had the authority." However, Bob was sufficiently self-critical to sense his own weakness; the vehemence of his attack decreased with the remoteness of the subject of discussion from a scientific field, and he would generally yield on purely administrative matters. Kent's vigorously asserted right to differ was always on a purely intellectual and objective plane; it was never personal or emotional. Almost strangely his discussions won friends for him; they never made enemies. To technical personnel (and many line officers) throughout the Army, Army Air Corps, and Navy he was just plain Bob Kent, widely known and beloved by all.

THE MAN

Whereas the character of every man is unique, some men are exceptionally unusual and different. Kent's character was indeed outstanding. Without any known exception, every man who knew Kent liked him; most loved him. He was intolerant of hypocrisy and pretense to the point of deprecating dignity. He was charitable, not only in a monetary way, but in entertaining the views of others and in recognizing the rights of others, and he was ever the champion of the underdog.

He loved social contacts, conversation, and exchanges of views, and was distinctly of the opinion that a moderate amount of alcoholic beverage lessened inhibitions and promoted good-fellowship. It was customary for the staff of the BRL to give a cocktail party and dinner for the BRL's Scientific Advisory Committee during its two-day visits, and Kent insisted (seriously) that the informal cocktail party was the best part of the meeting because the committee members criticized and advised more freely under the relaxing circumstances. On such occasions it was easy to get him to play the piano, particularly if others would sing, and he preferred the Whiffenpoof Song. Kent was also very fond of playing tennis and continued the sport as long as he could.

Kent died on February 3, 1961. His carefully written will left appropriate things to people who he thought would have a special interest in them. He left instructions that he be cremated and that there be no regular funeral ceremonies or sermon. Instead, there was to be piano music (Robert Schumann's "Traumerei," Bach's "Jesu, Joy of Man's Desiring"); a male quartet of friends to sing "Integer Vitae," with words by Quintus Horatius Flaccus; the reading of Ecclesiastes XII, verses 1–12 only (specifically omitting the 13th and 14th verses); the playing of Chopin's Sonata in B-flat minor (with the funeral march); and the singing of "Gaudeamus Igitur" by the male quartet.

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KEY TO ABBREVIATIONS

APG = Aberdeen Proving Ground
BRL = Ballistic Research Laboratories (U.S. Army)
OCO = Office of the Chief of Ordnance (U.S. Army)
OTN = Ordnance Technical Note

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Note: A list of more than 200 titles of official reports, written by Dr. Kent for the Ordnance Corps of the Army, is deposited in the Archives of the Academy. A few of these titles are listed below to exemplify the nature of the reports.

1919

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1921

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1923

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1924

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1925

Methods of determining velocities of small arms projectiles including also certain firings to establish the law of air resistance. Second Report, ballistic section, APG Report.

1926

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1927

A trigger spark apparatus for Aberdeen chronograph. Ballistic section, APG Report.

The effect of temperature on pressure and velocity of small arms. First Report, ballistic section, APG Report.

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1928

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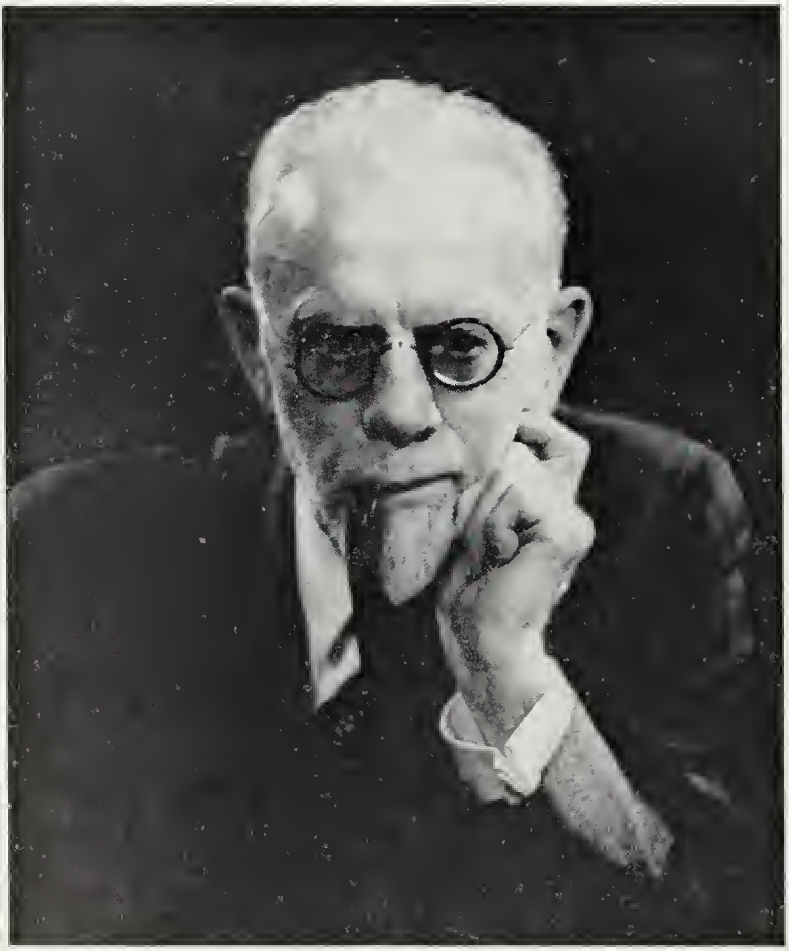

Charles A. Kraus.

CHARLES AUGUST KRAUS

August 15, 1875–June 27, 1967

BY RAYMOND M. FUOSS

On April 24, 1857, the *Mississippi* arrived at New York, after a 55-day voyage from Amsterdam. One of the passengers, Johann Heinrich Kraus, was then a boy of seventeen. He, his mother, and his sister were emigrating from Traubach in the Rhineland, because 1856 had been a poor wine year. They had sold their vineyard and put part of the proceeds into transatlantic fares. They went first to Scranton, Pennsylvania, where an uncle was living. Later, John Henry moved further west, to Knightsville, Indiana, a small coal mining town, and started farming nearby. He married Elizabeth Schaefer; on August 15, 1875, their son Charles August Kraus was born.

The early education of young Charles August was in a one-room country schoolhouse, about a mile from the farm; he once said that the school term varied from year to year, ranging from three to eight months, depending on weather, crops, and availability of teachers. By 1890, Kraus was ready to enter high school; the nearest was at Hayes City, about four miles distant. He did the commuting on horseback. In later reminiscences, he credited L. H. Gehman, the principal of the high school, and a graduate of Gettysburg College, for arousing his interest in science in general and in electrical phenomena in particular. He graduated from Hayes City High School in 1893, and decided to go to the University of Kansas to study electrical engineering. He could meet all the entrance requirements except the one calling for knowledge of French; he was fluent in German, but that was no help in satisfying a French requirement. He studied French during the summer of 1893, and as he put it, "managed to gain admission to the course in scientific French," which he passed "in creditable fashion," after being admitted to the University of Kansas in the fall of 1893.

At the University of Kansas, he entered the course in electrical engineering, which was headed by Lucian Ira Blake, who had received his doctorate in physics at the University of Berlin while Helmholtz, Kirkhoff, and Hertz were there. Probably Blake encouraged Kraus toward fundamental rather than applied science; in any case, Kraus wrote the following about his Kansas years in a brief autobiographical sketch (which unfortunately was never completed): "I became more interested in physics than in engineering, and by the beginning of the Junior year, was spending more time reading literature relating to physics than to engineering. I was familiarizing myself with the work of Van Ard, Crookes, and Hittorf, and, of course, with work on X-rays (discovered by Roentgen in 1895) and radioactivity, which came along a year later. I early became interested in the process whereby electricity was conducted through metals. At that time, little was known about it; the concept of electrons was just in the stage of being developed. Drude, in fact, wrote a book on metallic conductance in which he accounted for a good many properties of metals on the assumption that electrons were small, gas-like particles. The most significant result was the relation between electrical and thermal conductivity. In the spring of 1897, H. P. Cady, then a senior in chemistry in the University, published a paper (*J. Phys. Chem.*, 1:707 [1897]) in which he presented the first measurements on the conductance of electrolytes in liquid ammonia. Among other substances whose conductances he measured was metallic sodium. While the measurements were rough, they clearly showed that solutions of sodium were much better conductors than solutions of salts. Another most important observation was that the passage of current from solution to electrode is reversible. This paper was published in the spring of 1897; when I ran across it, I understood that light might be thrown on the conductance process of electrons through metal by a study of metals in liquid ammonia."

Here was the beginning of Kraus as a physical chemist. The record shows that his formal training in chemistry totaled one year of general chemistry and about a week of qualitative analysis. But as time went on, he became one of the outstanding figures in all of chemistry: his work on the reactions of compounds of carbon in liquid ammonia and the synthesis and properties of many metallo-organic compounds places him among the organic chemists; the researches on intermetallic compounds, on the oxides of sodium and potassium, on glasses, on the extraction of gallium and germanium from their ores, puts him among the inorganic chemists; when, as usually was the case, a method for analyzing one of his new compounds was not available, he designed one, thereby contributing to analytical chemistry. Researches on the properties of solutions, especially their conductance in a wide variety of solvents ranging from hydrocarbons to highly polar solvents, contributed to physical chemistry over a period of nearly seventy years. His field of research interest and activity was without question far broader than that of any of his contemporaries, embracing as it did the whole scope of chemistry.

Before leaving physics for chemistry, Kraus began research in spectroscopy as a college junior, with two instructors in the physics department at Kansas. Zeeman had reported in the March 1897 issue of *Philosophical Magazine* that the sodium lines from a source in a magnetic field were "broadened" (later work, of course, showed that they were split; Zeeman received the Nobel Prize in 1902 for this work). Zeeman had used a concave grating to observe the effect of the field on the spectral lines. About ten years before, Michelson at Case Institute had invented the interferometer. The Kansas physicists saw in the interferometer a more precise tool for studying intensities, and applied it to the magnetic effect with immediate success. Their paper in the *American Journal of Science,* dated April 15, 1897, showed that the percentage broadening was directly proportional to the field strength, as predicted by Lorentz from electromagnetic theory. Soon thereafter, the concept of electrons appeared. This early familiarity with subatomic phenomena was certainly the foundation of Kraus's later recognition of the solvated electron as the charge carrier in solutions of the alkali metals in liquid ammonia.

As Kraus recounted the story, it was Cady's work on solutions in liquid ammonia that catalyzed the transition of Kraus from physicist to chemist. The history of the work on ammonia is intrinsically interesting, and relevant to the subsequent development of the field by Kraus. In an address given by Cady before the Division of Physical and Inorganic Chemistry on April 15, 1936, at a meeting of the American Chemical Society at Kansas City, Cady said that "Professor Ira Remsen of Johns Hopkins started the work, though he never knew it." This start was a statement by Remsen in his *Inorganic Chemistry* that "ammonia forms many compounds with salts corresponding closely to those of water with the same salts." To continue quoting from Cady's address: "When I came to the University of Kansas in '94, I read everything available on water of crystallization and ammonia and its compounds, with special attention to liquid ammonia and the ammonia of crystallization. I made many of the compounds just to see what they looked like. The great similarity in every way of water and ammonia impressed itself more and more strongly upon me. I finally laid my information before Franklin with a suggestion that liquid ammonia might reasonably be expected to be an ionizing solvent. Franklin was instantly all enthusiasm. He persuaded Professor Bailey to invest a large part of the merger fund available to the chemistry department for research, in a cylinder of liquid ammonia. He made me some vacuum jacketed test tubes. There were no vacuum jacketed vessels on the market anywhere in the world in those days. Most unfortunately for me, Franklin was under contract to go to Central America for a gold mining company and left before the ammonia arrived. It was a heart-trying moment when I drew out some liquid ammonia into one of the vaccum tubes and tested it to see if it was a conductor of electricity. Finding the resistance to be very high, as I expected, I then dropped in a piece of potassium iodide, which Gore had shown to be soluble. I shall never again experience a thrill equal to that which I felt when the solution became a good conductor and electrolysis took place!"

On Franklin's return to Kansas, he and Kraus continued the work started there by Cady, who moved to Cornell to study phase rule problems with Bancroft. The first of a series of papers by Franklin and Kraus was entitled "Liquid Ammonia as a Solvent"; it appeared in 1898 in the *American Chemical Journal,* the immediate ancestor of the *Journal of the American Chemical Society.* In this paper they give the earlier history of ammonia solutions, tracing it back to Faraday, who had prepared liquid ammonia by heating dry "ammonio-chloride of calcium" in one leg of a U-tube, condensing the ammonia by its own pressure, and simultaneous refrigeration of the other leg, onto a substance whose solubility was to be investigated. Gore, using this method, reported solubilities of some 350 compounds in 1872. Weyl (1863) and Seeley (1871) described the blue solutions formed by sodium and potassium in ammonia. Joannis (1890) began the study of chemical reactions in ammonia. A second paper with Franklin on the "Determination of the Molecular Rise in the Boiling Point of Liquid Ammonia" also came out in 1898, the year Kraus received the degree of Bachelor of Science (Engineering) from the University of Kansas.

Kraus stayed at Kansas for a year's postgraduate work with Franklin, which culminated in a 36-page paper presenting conductance data for 25 compounds in liquid ammonia, covering a wide range of concentration. This was the first really broad survey of the field, and also represents a masterpiece of laboratory technique. All the intricate glassware required for quantitative manipulation of solutions at —33° in a closed system had to be made by hand from soda glass. (Kraus liked to tell his students in later days how he used to make Dewar flasks by sealing together two oil-lamp chimneys, and deplored the invention of Pyrex as the ruination of the art of glass blowing.) The work on conductance in liquid ammonia was continued for a year at Johns Hopkins (1899-1900), after which Kraus returned to Kansas for another year. He then moved to California, formally as instructor in physics, but the research papers published by Kraus during that period were also on liquid ammonia. One was a continuation of the work on conductance; 37 more compounds were studied. In addition to metallic salts, groups of organic compounds were investigated. It was found that many amides and nitrocompounds, insulators in the pure form, become electrolytic conductors in solution in liquid ammonia. This was pioneer work on general acid-base systems, antedating by many years the "modern" theory of acids and bases. The experimental results showed that the amides and nitrocompounds generated electrolyte when dissolved in ammonia; the latter acted as a base with respect to the solutes to produce salts. Many of these were as good conductors as ordinary salts such as sodium chloride. The parallel between aqueous and ammoniacal systems was clearly shown: the amide and ammonium ions in ammonia are the analogs of the hydroxyl and hydronium ions in water. During the California stay, pioneering work on conductance near the critical point in methanol and ethanol was also done. The significant finding was that the conductance showed no discontinuity at the critical point; furthermore, the vapor phase is also an electrolytic conductor.

About the turn of the century, A. A. Noyes founded the first school of physical chemistry in the United States at the Massachusetts Institute of Technology. Kraus joined the group as research assistant in 1904. His all-absorbing interest was research; at the insistence of Noyes, however, he took enough time off from research to pass the formal requirements for the degree of Doctor of Philosophy in 1908. He was then promoted to research associate and in 1912 to assistant professor of physical chemistry. The years at M.I.T. (1904-1914) were highly productive: the series of papers on the properties of solutions of metals in ammonia and in several amines are classical. The first paper, published in 1907 in the *Journal of the American Chemical Society,* is Contribution No. 18 from the Research Laboratory of Physical Chemistry of the Massachusetts Institute of Technology. At that time, it was known that sodium and potassium form blue solutions in ammonia, and that lithium and cesium dissolve in methylamine, but little quantitative information was available. Kraus determined the phase diagram for the sodium-ammonia system and defined the ranges of $Na–NH_3$ ratios where the two liquid phases were in equilibrium: a heavier, more dilute blue solution, and a lighter, concentrated solution, "presenting a characteristic bronze colored metallic appearance." The rate of reaction of sodium with ammonia to form sodamide was determined and the catalytic effect of solid amide was observed. The first clearly defined intermetallic compound, $NaPb_2$, was formed in liquid ammonia; electrolysis experiments showed that the lead was in the anion. Tin also was found to form a sodium compound which gave a red solution. In the second paper, it was shown that previously claimed solid compounds containing one atom of alkali metal and one molecule of ammonia did not exist; calcium, however, forms a hexammoniate which is a metallic conductor. Next, the vapor pressures of solutions of sodium in liquid ammonia were determined; in dilute solution, the molecular weight was found to approach the theoretical value of 23 for sodium, while at higher concentrations the pressure changes were considerably less than predicted by Raoult's law.

Papers IV and V, in 1908 and 1914, of the series "Solutions of Metals in Non-metallic Solvents," give the essential part of the explanation of the behavior of the metal-ammonia systems. For sodium solutions, for example, it was shown that the cation was the same sodium ion which was present in ammonia solutions of salts such as sodium chloride. The anion, however, was something quite new to solution chemistry. Kraus showed that it was "an electron, surrounded by an envelope of solvent molecules." The blue color of the solutions was due to the presence of solvated electrons, which are common to all metallic solutions. The identity of the anion with the electron also explained the perfect reversibility of electrodes in sodium-ammonia solutions: the negative carrier can pass into and out of solution without observable material effects at the interface between solution and insoluble electrodes such as platinum. The continuous transition from electrolytic to metallic conductor could be demonstrated by observations on these solutions. In order to determine the relative speeds of cation and solvated electron, the method of electromotive force of concentration cells was used, first because the Hittorf method could not be applied conveniently to metal-ammonia solutions, and second, because here was a system with perfectly reversible electrodes. These experiments showed that in dilute solutions the solvated electron behaved very much like a conventional anion, albeit a rather speedy one, while at higher concentrations "the negative electrons are free from the ammonia envelope for a fraction of the time, and then move with a speed comparable with that of the negative electrons in metals." This was the first demonstrated tunnel effect in liquids, about twenty years before the advent of wave mechanics.

A real tour de force of the M.I.T. days was the determination of the potential of the normal sodium electrode against the standard calomel electrode. G. N. Lewis was collecting the standard potentials of the metals as fundamental thermodynamic information concerning reactions; the alkali metals presented a seemingly impossible problem, because they react violently with water. Kraus solved the problem by a two-step process: he determined the potential of a 0.206 percent sodium amalgam against normal sodium ion in water as 2.1525 volts, and then determined the potential of the same amalgam against pure sodium in ethylamine as 0.8456 volt. The sum, 2.9981 volts, gave the desired standard potential of sodium. The design of the experiment and of the apparatus for performing it is characteristic of all of Kraus's researches.

It was also during the M.I.T. period that Kraus first became interested in the general problem of electrolytic behavior. In collaboration with W. C. Bray, conductance data for a wide variety of electrolytes in ammonia, sulfur dioxide, the hydrogen halides, and in many other inorganic and organic solvents were critically reviewed, and analyzed on the basis of the mass action equation. The classical Arrhenius form of the equation was modified empirically to give a form which served to reproduce experimental results by the use of two more parameters. The paper concludes with the prophetic comment: "An explanation of the observed deviations from the mass-action law is to be looked for in the interacting forces due to the presence of charged particles (ions) throughout the body of the solution." Here, in 1912, was a qualitative anticipation of the interionic force theory of Debye and Hückel in 1923.

Preliminary work in another field, which was later to become a major activity for Kraus, also began at M.I.T.: the synthesis, structure, and properties of metallo-organic compounds, in which metallic atoms are covalently bonded to carbon. The alkyl mercury halides were found to be soluble in liquid ammonia. On electrolysis, the deposit at the cathode was shown to be $HgCH_3$, a completely new type of chemical compound: although it contained a methyl group, its properties were those of a metal!

After ten years at the Massachusetts Institute of Technology, Kraus was invited to Clark University as Professor of Chemistry and Director of the Chemical Laboratory in 1914. Until then, Kraus had had only three collaborators: E. C. Franklin at Kansas, and G. N. Lewis and W. C. Bray at M.I.T. The bulk of his research had been planned by himself, and he did the experimental work himself, with no research assistants or graduate students. During the sixteen years since his graduation as an engineer from the University of Kansas in 1898, Kraus had nevertheless established himself as the outstanding authority on the physical properties of solutions in liquid ammonia, and had already begun pioneering work on chemical reactions in ammonia, especially those involving metal-metal and metal-carbon bonds. At Clark, Kraus began to build a graduate department of chemistry. Progress was slow initially, partly as a consequence of World War I, during which Kraus was an active consultant to the U.S. Chemical Warfare Service and to the U.S. Bureau of Mines. This involved direction and advice on research at the federal laboratories as well as diversion of graduate student work to research applied to war-generated problems. (This pattern, incidentally, was to be repeated again several decades later.) Also graduate work in chemistry at Clark was essentially a new venture; it took time for chemistry students graduating from other colleges to realize that at Clark there was developing a school of research in the broad field of general chemistry, combining in each graduate research project physical chemistry with reaction chemistry in non-aqueous solvents. Kraus gradually built up a group of about a dozen graduate students, large enough to be highly productive and small enough for each member to get the benefit of "the King's" advice and direction daily. The students learned how to plan an experimental approach to a problem, and to design and build apparatus for their experiments. This close contact with his students which began at Clark was maintained throughout Kraus's entire career. A student who did his dissertation research with Kraus graduated as much more than one who had fulfilled the formal requirements for the degree of Ph.D. in Chemistry: he was one who had learned research thinking.

The publication record tells the story: only three papers between 1914 and 1921, one on the design of a mercury vapor pump which required only a water-jet as backing pump, and two on the extrapolation of conductance data. But in the next few years, about thirty papers appeared: some continuing the investigation of solutions of metals in ammonia, some on the chemistry of reactions in ammonia, and others in which the scope of the work on conductance was broadened to include aqueous solutions and a variety of organic solvents. In all, fourteen graduate students are named as co-authors in this series of papers from Clark, and this does not complete the roster of the Clark students, because about a dozen moved with Kraus to Brown in 1924, to complete their dissertation researches there.

The fifth paper on "Solutions of Metals in Non-metallic Solvents" had appeared in 1914, as one of the last of the M.I.T. papers. After arriving at Clark, Kraus continued experimental work on his own; the sixth paper of the series was published in 1921. By comparing the conductance of lithium, sodium, and potassium in liquid ammonia, it became clear that the negatively charged carrier was the same for all three metals. The minimum in equivalent conductance around 0.05 normal marks the transition from electrolytic conductance where the current is carried by metallic cations and by solvated electrons, to metallic conductance where, with increasing concentration of metal, relatively more and more current is carried by the negative carrier which in the more concentrated solutions moves essentially like the free electron in metals. A saturated solution of sodium in ammonia has about half the specific conductivity of mercury at 0°. The conductance of concentrated solutions (from concentrations below that corresponding to the minimum in conductance to saturation) of sodium and potassium was determined by W. W. Lucasse. The temperature coefficients confirmed the transition from electrolytic to electronic conduction, as the concentration passed through the range at the conductance minimum: at dilutions greater than about four liters per atom, the coefficient was about 1.55 percent; with increasing concentration, it goes through a maximum of 3.6 percent at one liter per atom, and then rapidly drops to about 0.07 percent near saturation. The phase diagram for the sodium-ammonia system was determined very precisely. Later, it was found that the temperature coefficient of potassium solutions in liquid ammonia, on the high concentration side of the minimum, was quite similar to that for sodium, an observation which was again confirmation of the hypothesis that the electron was the primary current carrier in the metal-ammonia solutions.

All of the work on the physical chemistry of solutions of the alkali metals in liquid ammonia was to have important consequences for reaction chemistry. The physical work had clearly demonstrated the presence of electrons in these solutions. Now electrons are obviously the most potent reducing substance which can exist, and in liquid ammonia, one has a medium in which the dissolved electrons are stable. (In the presence of certain solid catalytic surfaces, electrons will react slowly with ammonia to form gaseous hydrogen and the amide ion. This reaction is, of course, the analog of the reaction between electrons and water to form hydrogen and hydroxide ion; the latter is, however, by many orders of magnitude, a much faster reaction.) With George F. White, professor of organic chemistry at Clark, a preliminary study of reduction in ammonia solutions was made. This was a most fruitful research, and represented the beginning of a long series of subsequent chemical investigations. The general pattern of the reactions was an initial step, in which the electronegative constituent or group of an organic compound reacted with the electron to produce the corresponding anion; the residue of the reacting molecule was left as a free radical. Further steps of the reaction then depended on the reactivity and stability of the free radical. For example, triphenylchloromethane reacts first to give chloride ion and triphenylmethyl. The free radical then adds an electron, to give the triphenylmethide ion, so the over-all stoichiometric reaction is $2Na + Ph_3CCl \rightarrow NaCl + NaCPh_3$. The actual reaction comprises two steps: $e^- + Ph_3CCl \rightarrow Ph_3C\cdot + Cl^-$; $Ph_3C\cdot + e^- \rightarrow Ph_3C^-$. The sodium cation takes no part in the reaction. On the other hand, if the electronegative group is accompanied by a group which gives a very reactive radical, ammonolysis occurs. So, for example, propyl iodide gives iodide ion and propyl radical; the latter does not accept an electron to form a propide ion but reacts with solvent ammonia to give propyl amine and propane.

Kraus's earlier interest in intermetallic compounds and organo-metallic compounds continued at Clark. The former included compounds of sodium with tellurium, antimony, and lead; again, liquid ammonia proved to be the unique solvent in which these materials could be prepared and characterized. Vapor pressure measurements showed that Raoult's law was very nearly followed by sodium-tellurium complexes in ammonia, and that two atoms of sodium are present for each complex telluride in solution; the conclusion was that the latter carried two negative charges. The polytellurides thus turned out to be the predictable analogs of the polysulfides previously known in aqueous systems as polyatomic clusters of fixed valence. At the same time, research began on compounds in which the anion was a metal covalently bonded to carbon, with tin as the first metal to be studied. Trimethylstannane was one of the first of a series of substituted metallic hydrides to be prepared. Sodium in ammonia reacts with trimethylchlorostannane to give the salt sodium trimethylstannide; this in turn reacts with ammonium chloride, the ammonia analog of hydrochloric acid in water, to generate Me_3SnH, plus sodium chloride and ammonia. This parallel, exemplified by the following equations, served as the guiding principle for much of the metallo-organic chemistry developed by Kraus: $Me_3Sn^- + NH_4^+ \rightarrow Me_3SnH + NH_3$; $X^- + H_3O^+ \rightarrow XH + H_2O$ where X^- is a strongly basic anion such as acetate or methoxide. It was shown that trimethylstannyl chloride was practically a nonconductor in the pure state, or in aprotic solvents such as ethers or nitrocompounds; in basic solvents, such as alcohols or amines, however, its conductance resembled that of the hydrogen halides in these solvents. These results suggested that solvolysis produced the conducting species.

The experience in metallo-organic chemistry led to a most important technological development, the practical synthesis of tetraethyl lead. At the end of an exhaustive systematic search, the Kittering-Midgley research team in 1921 reached this compound as the theoretically ideal antiknock agent for gasoline. But $PbEt_4$ was at best a laboratory curiosity: it could be made by the Grignard reaction from lead tetrachloride (which is also a rare specimen) or by reaction between sodium-lead alloy and ethyl iodide, but iodides were too expensive to be considered as raw materials for a gasoline additive. Midgley found that ethyl bromide could replace ethyl iodide, at the price of slower reaction rate, lower yield, and enhanced corrosion problems. Kraus was called in as a consultant in 1922. Knowing that tin and lead are similar in many ways, and with his broad background of knowledge in the chemistry of tin compounds, Kraus started a group at Clark (Callis, Rosen, and Greer) on a summer research project aimed at an economical synthesis of tetraethyl lead, starting with the relatively inexpensive ethyl chloride as the source of ethyl groups. They discovered that ethyl chloride would react with sodium-lead alloys, and that one alloy in particular, the one which had the equi-atomic composition NaPb, would give very high yields of the desired antiknock agent in a rapid and controllable reaction. This discovery led to the formation of the Ethyl Corporation in 1924 to produce lead tetraethyl in commercial quantities.

As already mentioned, Kraus continued work on conductance of metals in liquid ammonia at Clark. It was during this period that he extended his interest to a wider variety of electrolytic systems. Prompted by the desire to attain high precision in the measurement of electrolytic conductance, he reexamined the fundamental standards, with the result that a discrepancy of nearly half a percent between the relative conductances of 0.1 and 0.01 normal potassium chloride solutions in water, the classical (1898) standards of Kohlrausch, was discovered. This led to a quantitatively precise evaluation of the conductance of standard solutions of potassium chloride, and to the establishment of the Kraus and Parker calibration solutions. These standards were internationally accepted, and remained in general use by research workers for well over a decade, until the introduction of the Jones and Bradshaw demal solutions in 1934. The latter were no more precisely defined than the standard solutions of Kraus and Parker, but had the advantage of being defined in terms of concentration units which, unlike normalities (moles per liter of solution), were independent of temperature. Then, using their standards, Kraus and Parker determined the conductance of iodic acid in water down to a concentration of 5×10^{-5} normal, using a quartz cell, and water purified to a conductance as low as 0.1×10^{-6}. The work gave a most pericse value for the limiting conductance of the hydrogen ion; the value was slightly revised later, after the Debye-Hückel-Onsager and Bjerrum-Fuoss-Kraus theories suggested a somewhat different method of extrapolating to infinite dilution.

A beginning of the later very broad program of investigation of electrolytic conductance was made at Clark, starting with solutions in propanol, amyl alcohol, and phenol. These are all solvents of lower dielectric constant than water. Salts such as iodides, which are normal "strong" electrolytes in water, were found to behave like weak electrolytes in these solvents. Specific differences appeared between salts which gave similar behavior in water: for example, sodium iodide was a much weaker electrolyte in phenol than tetramethylammonium iodide. A minimum in equivalent conductance for Me_4NI in phenol around 0.03 normal was also observed. This early work showed the existence of a whole new field of electrochemical research, that of the specific interactions between ions and ions, and between ions and solvent molecules; this field was to be extensively and intensively worked in the later years at Brown. Another interesting system was explored in a preliminary way at Clark: ordinary soda-lime glass was found to be an electrolytic conductor at temperatures above about 270°. The current was carried mostly by the sodium ions, which could be replaced by other ions such as lithium or silver by electrolysis from amalgams or molten salts.

Based on his many researches on conductance, and his comprehensive knowledge of the literature on the subject, Kraus wrote in 1922 the American Chemical Society Monograph entitled *The Properties of Electrically Conducting Systems.* Modern theory has changed the interpretation of conductance data since 1922 and many more systems have been studied since Kraus wrote the monograph, but even so, it remains the most comprehensive work available on the subject. And it should be added that many of the subsequent advances and expansions in the field of electrolytic conductance originated in the laboratories of Kraus and of his students. Recognition was given Kraus for his outstanding work in nonaqueous solutions by the New York Section of the American Chemical Society with the award of the Nichols Medal in 1923.

The following year (1924), Kraus was invited to Brown University as Professor of Chemistry and Director of Chemical Research. He was to remain at Brown for many years: he became Emeritus Professor in 1946 at the age of seventy-one, but continued active direction of research until his middle eighties, when failing vision began to confine him to his home. His research activity started in the Newport Rogers Laboratory, the second floor of a wood and brick building on the Brown campus. This served for thirteen years; in 1938 the research group moved to the then new Metcalf Research Laboratory, which in layout and in many functional details was designed by Kraus. Research at Newport Rogers and at Metcalf was concentrated in the three fields in which Kraus had previously done pioneer work at Massachusetts Institute of Technology and at Clark: physical chemistry of liquid ammonia systems, the chemistry of metallo-organic compounds (especially those containing silicon, germanium, tin, boron, aluminum, and gallium), and conductance in non-aqueous solvents.

The liquid ammonia work included further studies on metal-ammonia systems, and the conductance of a variety of salts which could only exist in ammonia. They included the amides of sodium and potassium, sodium salts of the pattern $NaMPh_3$ where M is carbon, tin and germanium, sodium trimethylstannide, salts of phenols and thiophenols, and metal ketyls. All of these react violently with air or water, so it was necessary to design apparatus and techniques by which quantitative measurements of high precision could be made in completely closed systems. Thermochemistry in liquid ammonia called for the design of a precision calorimeter which would function at —33°; this was achieved by measuring quantitatively the amount of ammonia evaporated by exothermic reactions occurring at the boiling point of ammonia. It was found that the heats of solution of inorganic salts were in general positive, while that of metallic sodium was negative. Heats accompanying a variety of reactions were determined, including some of those involving intermetallic compounds. Precision in conductance equaling that obtained in aqueous solutions was achieved by Hnizda (1949), who measured four alkali halides over a wide range of concentration.

The chemistry of compounds in which a bond is formed between carbon and a metallic atom is an especially interesting field. Kraus contributed about forty research publications to the subject after coming to Brown. Silicon, germanium, tin, and lead are all analogs of carbon, in that they are tetravalent. The series carbon to lead represents a transition from nonmetal to metal. Germanium, halfway through the series, was investigated in depth. It had been a relatively rare element until Kraus devised a method for extracting it from an ore (germanite) discovered in Tsumeb, South West Africa, by Schneiderhöhn in 1920. The method depended on the reduction of germanic sulfide to germanous sulfide by ammonia at 825°, and sublimation of the latter. This dry process gave almost quantitative recovery, and was far superior to the previous methods based on solvent extraction. With an adequate supply of starting material thereby made available, a wide variety of alkyl and aryl derivatives of germanium were made. Germanium was found to form a normal hydride GeH_4, which gives a sodium salt in liquid ammonia; that salt slowly decomposes to produce sodium germanide, NaGe. This, in turn, reacts with ammonium bromide to form a polymeric material in which the ratio of germanium to hydrogen is one-to-one. The ore from which the germanium was obtained also contains appreciable quantities of another rare element, gallium, which is the next higher analog of boron and aluminum. A method of extracting gallium from the ore residue after recovery of the germanium was developed, and the product was used as the starting material for an extensive study of the compounds of gallium. Metallo-organic compounds of aluminum and tin were also investigated: the alkylaluminum halides later found application in combination with titanium chloride as initators for ethylene polymerization. Kraus showed that carbon was not unique in its ability to form chains by the synthesis of a compound in which five tin atoms were linked together. Much of our knowledge of the elements of the third and fourth groups of the periodic table had its origin in Kraus's laboratory at Brown. Summaries of this work were published in 1927 and 1928 in Volumes I and II of *Survey of American Chemistry.*

The third major research field was the properties of electrolytic solutions (especially their conductance) in non-aqueous solvents; over seventy papers on this subject were presented. In fact, from the early 1930s, electrolytes became more and more the dominant theme of the researches directed by Kraus. He had pointed out quite early the fact that the traditional physical chemistry of solutions was largely based on observations on solutions in water, which in many ways is an exceptional solvent. Kraus correctly insisted that a general theory of electrolytes must be based on information on a wide variety of solvents, ranging from non-polar to polar, and including aprotoic as well as hydrogen bonding solvents. Prior to his sojourn at Brown, he had investigated conductance in liquid ammonia and several of the alcohols. The attack on the general problem began in 1930 with a study by Fuoss of mixtures of dioxane and water, and of benzene and ethylene dichloride. With the first mixtures, the range of dielectric constants from about 80 down to 2.2 in protoic solvents is accessible, and with the second, the range from 10.2 to 2.3 can be covered in media which contain no bonding hydrogen atoms. The fundamental significance of dielectric constant was quantitatively demonstrated, and the primary control of ion pair formation through dielectric constant was established. By combining the Debye-Hückel theory of long-range ionic interaction with the Bjerrum hypothesis of ion pair formation, Fuoss and Kraus presented the first general and comprehensive treatment of electrolytic solutions. This theory, modified and improved as time went on, was the guiding principle for a long series of conductance studies, in which molecular structure of solute and solvent was correlated with the macroscopically observed properties of the solutions. The formation of triple ion clusters was shown to be the cause of the minima observed in conductance curves; dependence of the location of the minima on dielectric constant was theoretically established. Quadrupole clusters were shown to exist by precise measurements of freezing points in benzene and in dioxane. Direct physical evidence for the existence of stable ion pairs was given by dielectric constant measurements in benzene solutions of quaternary salts: dipole moments of the order of 15-25 Debyes were observed. Solvents used in the conductance work included dioxane, benzene, ethylene chloride, ethylene bromide, anisol, nitrobenzene, pyridine, acetone, methyl bromide, methylene chloride, and bromine. The problem of determining single ion conductances in nonaqueous solvents was solved by Fowler and Kraus by the synthesis of tetrabutylammonium triphenylborofluoride. The ions of this salt have practically equal volumes, and therefore the single ion conductance of each is given by half the limiting conductance of the salt. Given the conductance of one anion (or cation), the conductances of other single ions are of course determined by Kohlrausch's law of independent mobilities at infinite dilution. Among the most important of Kraus's publications on electrolytes are the reviews which he wrote at about five-year intervals; these served as supplements which kept the 1922 monograph on conductance up to date.

Kraus's eminence in research was recognized by a number of awards, beginning with the Nichols Medal in 1923. In 1935 he received the Willard Gibbs Medal, with the citation: "Substituting ammonia for the common solvent water, Dr. Kraus has been able to arrive at conclusions which have added immensely to a better understanding of the behavior of chemical substances in any solutions." The Theodore William Richards Medal was awarded in 1936, "for conspicuous achievements in the chemistry of liquid ammonia and electrolytes." In 1938 he received the Franklin Medal, which is "awarded annually to those workers in physical science or technology, without regard to country, whose efforts have done most to advance a knowledge of physical science or its applications." The citation read: "To Dr. Kraus, in recognition of his numerous researches in chemistry and electrochemistry, particularly his valuable contributions to the knowledge of reactions in liquid ammonia, of the chemical behavior of metals, and of the properties of electrolytic solutions." He was President of the American Chemical Society in 1939. The Naval Distinguished Public Service Award, the Navy's highest civilian award, was given to Kraus in 1948 for direction of naval research at the Metcalf Laboratory during the war. The American Chemical Society honored Kraus in 1950 with its most outstanding mark of recognition, the Priestley Medal. Honorary doctorates were conferred by Brown University, Clark University, Kalamazoo College, Colgate University, and Indiana University.

Kraus was a member of many professional and honorary societies. These included the National Academy of Sciences, American Academy of Sciences, American Philosophical Society, American Chemical Society, American Physical Society, American Association of University Professors, American

Association for the Advancement of Science, Faraday Society, Washington Academy of Sciences, Phi Beta Kappa, Sigma Xi, Phi Lambda Upsilon, and Alpha Chi Sigma. He was an Honorary Fellow of the Franklin Institute.

Kraus was elected to membership in the National Academy of Sciences in 1925. He was a member of the Council of the Academy for 1939-1942, a member of the Executive Committee of the Council of the Academy during 1940-1942, chairman of the Section of Chemistry for 1935-1938, and a member of the Board of Directors of the Wolcott Gibbs Fund for 1938-1960. He was chairman of the Division of Chemistry and Chemical Technology of the National Research Council in 1932-1933 and Field Secretary of the Fellowship Board in Physics, Chemistry, and Mathematics of the National Research Council in 1934-1935.

Kraus died June 27, 1967, at Providence, Rhode Island. His wife, Frederica Feitshans Kraus, predeceased him in 1963. He is survived by three sons, C. Newton Kraus, Philip B. Kraus, and Douglas L. Kraus, one daughter, Mrs. H. K. Hartline, thirteen grandchildren, and several great-grandchildren.

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KEY TO ABBREVIATIONS

Am. Chem. J. = American Chemical Journal
Chem. Eng. News = Chemical and Engineering News
Chem. Rev. = Chemical Reviews
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Chem. Educ. = Journal of Chemical Education
J. Chem. Phys. = Journal of Chemical Physics
J. Franklin Inst. = Journal of the Franklin Institute
J. Phys. Chem. = Journal of Physical Chemistry
Phys. Rev. = Physical Review
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Surv. Am. Chem. = Survey of American Chemistry
Trans. Am. Electrochem. Soc. = Transactions of the American Electrochemical Society (later, Transactions of the Electrochemical Society)
Trans. Faraday Soc. = Transactions of the Faraday Society

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Kenneth F. Maxcy

KENNETH FULLER MAXCY

July 27, 1889–December 12, 1966

BY W. BARRY WOOD, JR. AND
MARY LEE WOOD

THE PROFESSIONAL LIFE of Kenneth Fuller Maxcy was marked by a singleness of purpose in his chosen field. He was an epidemiologist at heart from the time of his graduation from medical school; and many years before his death (December 12, 1966) he became generally recognized as one of the leading epidemiologists of his time.

He was born in Saco, Maine, on July 27, 1889, to Estelle Abbey Gilpatric and Frederic Edward Maxcy, a physician, who moved his family a year later to Washington, D.C., to establish a general practice.

During his years at Washington's Central High School, Kenneth Maxcy was an ardent athlete and a fine scholar, although he showed no special predilection for scientific studies at that time. He graduated from George Washington University with honors in 1911, having been three times elected president of his class.

While in college he took a premedical course, continued his interest in football and track (he is reported to have once run a dead heat in a mile race), and spent his summers on expeditions to the western part of the country with surveying teams from the Forest and Indian Services. During these months he delighted in mountain climbing, camping, and surveying as an employee of the federal government, and the knowledge he gained of the great variety of terrain and wild life of his own country later proved invaluable to him as an epidemiologist.

As was often the case with doctors those days, Dr. Frederic Maxcy's office was under the same roof as the home; his children were familiar with the comings and goings of patients, and often accompanied him as he drove out to make house calls. Since the age of fifteen or so, Kenneth had helped his father in many ways, and upon finishing college it was a natural decision for him to enter medical school at the Johns Hopkins University in nearby Baltimore, where he received his M.D. degree in 1915.

A year's medical internship and an assistant residency in pediatrics at the Johns Hopkins Hospital were followed by an appointment in medicine at the Henry Ford Hospital in Detroit. During the pediatric residency, he had met Gertrude McClellan, R.N., who had come to Baltimore to teach pediatric nursing at the Harriet Lane Home, the children's service of the Johns Hopkins Hospital. In 1918 they were married.

With the entrance of the United States into World War I, Dr. Maxcy joined the Army Medical Corps and served for two years as a captain. One of his assignments was with a special unit to relieve the medical staff in camps where contagious disease was unusually prevalent. This may have stimulated his interest in epidemiology; or perhaps it was his subsequent assignment to the laboratory service and his encounter with the influenza pandemic. At the war's end he enrolled in the newly established School of Hygiene and Public Health at Johns Hopkins, and received his doctorate of public health in 1921. Before graduation, he was awarded a Rockefeller Foundation fellowship, which enabled him to go to Topeka to organize the Public Health Laboratory for the State of Kansas.

With an excellent background of both medical and public health training, Dr. Maxcy now entered the U.S. Public Health Service, where he remained for eight years, attaining the rank of Surgeon. He was first sent to the South to study the distribution and public health aspects of malaria. This field study resulted in a series of important scientific papers on the diagnosis and treatment of the disease.

While engaged in the malaria work, Dr. Maxcy first became interested in rickettsial infections. He made a careful clinical, laboratory, and epidemiological investigation of the 197 cases of typhus fever reported over several years in two communities in the southeastern United States, and concluded that a mild form of the disease, indistinguishable from Brill's disease but distinct from classical louse-borne typhus, was endemic in the area. His chief contribution to the discovery of "endemic typhus" was his bold postulation that the natural reservoir of the disease is in wild rodents and that man becomes incidentally infected from their insect parasites. At the time it was assumed that typhus was always transmitted from man to man by body lice, as had been clearly established in the case of European typhus. He was subsequently assigned to the Hygienic Laboratory in Washington (forerunner of the National Institutes of Health) where he initiated laboratory studies in furtherance of this hypothesis. He showed that the reaction of the guinea pig to inoculation of blood from endemic typhus patients was like that to Mexican typhus and different from Old World typhus. His remarkable epidemiological inferences were fully confirmed in 1931 by Dyer, Rumreich, and Badger, who isolated rickettsiae from rat fleas in Baltimore, and by Mooser, Castaneda, and Zinsser, who demonstrated them in the brains of rats trapped in Mexico City.

Many years later (1943) as a member of the United States Army's Typhus Commission in World War II, Dr. Maxcy collaborated in the direction of another rickettsial field study of "scrub typhus" (tsutsugamushi disease) in New Guinea. These investigations were reported in a monograph which he published with Dr. Francis G. Blake, and which won for them the Typhus Commission Medal in 1945.

In 1929, at the urging of his old friend and former teacher, Dr. Wade Hampton Frost, Dr. Maxcy left the Public Health Service to become Professor of Bacteriology and Preventive Medicine at the University of Virginia, where he stayed for seven years. The investigations of endemic typhus continued, and papers were contributed also on undulant fever and on a serological survey for syphilis. Always, Dr. Maxcy reported promptly and accurately on the results of his studies, and was most generous in the sharing of his time and his ideas with students and colleagues.

In 1936 the University of Minnesota invited Dr. Maxcy to become Professor of Public Health and Preventive Medicine and to organize its new School of Public Health. His work in this position was cut short after one year, however, by his recall to his alma mater, Johns Hopkins, to teach bacteriology. In 1938 he became Professor of Epidemiology, and remained in this post until his retirement in 1954, when he became Professor Emeritus.

At the annual meeting of the American Public Health Association in 1952, the William T. Sedgwick Memorial Medal was awarded to Dr. Maxcy. In making the presentation, Dr. Karl F. Meyer said in part:

"Under his guidance, teaching and research in epidemiology has, through the years, undergone a subtle transition. From a descriptive science overshadowed by an anthropocentric point of view it has become a biologic science, under the impact of teachings of Theobald Smith, whom the Association honored with the Sedgwick Award in 1930. This reorientation is fully documented in the masterful chapters on epidemiology that Dr. Maxcy has presented in some of the recently published textbooks on preventive medicine and medical microbiology.

These basic principles might have remained written documents had not his ability created interest among his students and stimulted them to analyze problems and to use their biologic information in reaching an explanation of the ecologic phenomena at hand. This major and real contribution rests on the friendly and remarkably tolerant, but critical, fashion in which he incited the thoughts of others. In consequence, he has sent scores of inquisitive young men and women from his classes eager to emulate his example of clear thinking before acting. . . . To find the qualities of the competent scientific investigator so well combined with the qualities of the teacher who understands learning is not common."

Dr. Maxcy's experienced advice was continually sought by organizations concerned with epidemiological problems. He served as a Scientific Director of the International Health Division of the Rockefeller Foundation for many years, and was a valued adviser of the American Red Cross. For more than fifteen years he was a member of the Armed Forces Epidemiological Board and the Research and Development Board of the National Military Establishment. Perhaps most significant of all, his department of epidemiology in the Johns Hopkins University School of Hygiene and Public Health was selected by the National Foundation for Infantile Paralysis for the establishment of a poliomyelitis laboratory. Here he organized and directed investigations which led to a better understanding and eventual control of this dreaded infection. In the 1940s he published reviews of the epidemiology of poliomyelitis in which he stressed the evidence that effective transmission was directly from person to person and not by such routes as water, sewage, or flies as was commonly thought.

Dr. Maxcy served as chairman of the Committee on Research and Standards for the American Public Health Association from 1939 to 1946, and was a member of the Governing Council. When he shouldered the task of editing a new edition of Rosenau's book, *Preventive Medicine and Hygiene,* in 1949, the latest (sixth) edition was fifteen years old. Revisions and additions had to be extensive and many months of painstaking labor went into the preparation of the seventh and eighth editions of the book. He was associated with two medical journals: in the 1930s with the *American Journal of Medical Sciences*, as editor of the section on hygiene and public health, to which he contributed several review papers; and the *American Journal of Hygiene,* as chairman of the editorial board. Upon his retirement a volume of the latter journal was dedicated to him and later one of his early typhus papers was reprinted by this journal.

He had an interest, stemming probably from World War I, in meningococcus infections. In his departmental laboratories Phair and Schoenbach did a series of studies on this problem as encountered in World War II. Typhoid, shigellosis, and diphtheria were among the other problems on which he and his students published useful work. He was chairman of a committee of the National Academy of Sciences which reviewed the issue of water fluoridation for preventing dental caries and recommended its further adoption.

Dr. Maxcy met the difficulties of debilitating Parkinson's disease during the last years of his life with a cheerful and remarkable fortitude, sustained by his devoted wife and the affectionate concern of many of his colleagues and former students. He is survived by his wife and their three children, Kenneth Fuller Maxcy, Jr., of Pittsburgh, Frederic Reynolds Maxcy of Ellicott City, and Selina Maxcy Wolf of Baltimore.

Despite his many accomplishments in public service, teaching, and research, Dr. Maxcy will undoubtedly go down in medical history as the "discoverer" of murine (flea-borne) typhus. Indeed, his epidemiological predictions regarding this disease were, in many ways, just as remarkable as the classic studies of John Snow on cholera and the Broad Street pump.

HONORS AND DISTINCTIONS

SPECIAL POSITIONS

Scientific Director, International Health Division, Rockefeller Institute, 1937-1940, 1942-1945, 1948-1951
Board of Trustees of International Polio Congress, 1951
Consultant to Secretary of War, Army Epidemiological Board, 1941-1945
Member, National Advisory Health Council, 1942-1946
Consultant, Research and Development Board of National Military Establishment, 1946-1957
Member, Executive Committee, Advisory Board on Health Services, American Red Cross, 1945-1948
Member, Medical Advisory Committee, National Foundation for Infantile Paralysis, 1940-1948
Chairman, Committee on Research and Standards, American Public Health Association, 1939-1946
Editorial Board, *American Journal of Hygiene*
WHO Advisory Panel on Environmental Medicine and Sanitation, 1951

HONORS

Phi Beta Kappa
Alpha Omega Alpha
Delta Omega
Sigma Xi
Sedgwick Award of American Public Health Association, 1952
National Academy of Sciences, 1952

SOCIETIES

Fellow of American Public Health Association
Fellow of American Association for Advancement of Science
Association of American Physicians
American Society of Epidemiologists
Fellow of Royal Medical Society of London

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KEY TO ABBREVIATIONS

Am. J. Hyg. = American Journal of Hygiene
Am. J. Med. Sci. = American Journal of the Medical Sciences
Am. J. Public Health = American Journal of Public Health
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Am. Water Works Assoc. = Journal of the American Water Works Association
J. Infect. Diseases = Journal of Infectious Diseases
Public Health Rept. = Public Health Reports
Southern Med. J. = Southern Medical Journal

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1926

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C. E. Kenneth Mees

CHARLES EDWARD KENNETH MEES

May 26, 1882–August 15, 1960

BY HANS T. CLARKE

PRIOR TO THE INTRODUCTION of commercial photographic dry plates in 1877, and for many years thereafter, little attention was devoted to the nature of the photographic process. Abney had in 1874 studied the quantitative relationships between exposure to light, development, and the deposition of metallic silver from its bromide, but scant notice was taken of his findings. The significant contributions to the theory of photography made by Hurter and Driffield in 1890 were likewise long neglected. It was well into the present century when C. E. K. Mees and his school friend S. E. Sheppard revived the subject in a fruitful association, ended only by death, dedicated to research and development in the whole field.

Kenneth Mees was born in England on May 26, 1882, at Wellingborough, Northamptonshire. His father was a Wesleyan minister descended from a long line of clergymen of that denomination. As clerics of that church were customarily transferred to other pastorates every three years, the child's early development lacked firm roots outside his immediate family. By the time he was ten he had lived successively in Ipswich, Colchester, Hythe, and Barnet. He first went to school at the age of seven, having been taught the elementary subjects by his parents, and having already become an avid reader, largely of the religious and historical works available in his father's library.

In his privately printed personal memoir, "Myself and My Journey down the River of Time," he describes his early schooling as "little bits of history, little bits of grammar and English, a little arithmetic and, as was usual in England at that time, quite a little Latin grammar." It is clear that he did not enjoy this fare; he apparently had difficulty with classwork, and not until he was ten was it discovered that he was short-sighted. At the school which he attended at that age he was for the first time exposed to chemistry, and although it was badly taught it gave him a new and exciting experience, namely, the demonstration of a chemical experiment. This was the preparation of chlorine, which he was allowed to repeat with his own hands. The effect upon him was described in his own words: "By that time I had fallen in love, completely in love, with science. This was different from all the other stuff; it was about real things—things that you could touch and handle, that you could measure. I began to see at once, it seemed to me, what a marvelous thing science was."

Mees then went to Kingswood, a school established by John Wesley for the sons of Wesleyan ministers. It believed in classics and mathematics but did not really approve of natural science. It nevertheless had a laboratory and Mees persuaded the science master to give him the key to it in return for keeping it clean. Here he spent all his spare time, instead of in athletic activities for which he was physically and temperamentally ill adapted. He tells us: "This science became to me a sort of religion. I was brought up in a religious atmosphere; my father was a minister, my school was a religious school, I had lots of training in the Bible, and I am bound to say that I classified most of the Bible with the Latin grammar as a thing that had little relation to fact. But at the same time I saw a purpose in being on earth, and that was to increase knowledge."

Not content with this extracurricular laboratory experience, which far exceeded that afforded to his schoolfellows, Mees organized a laboratory in his home, where during the vacations he could pursue his passion for chemistry by experimentation as well as by reading.

At Kingswood the food was poor and the boy suffered from dyspepsia, soon complicated by rheumatism which he found (although this was before the days of pH) could be relieved by hot baths in dilute sodium hydroxide, whereas sodium carbonate was ineffective. In view of his condition his father transferred him, at the age of fifteen, to Harrogate College, a private school where the food was better and the rooms were warmer. Here his health improved; and though the instruction was inferior, the training that he had acquired at Kingswood and at home enabled him to pass without difficulty, in June 1898, the matriculation examination of the University of London. The family then moved to Croydon, Mees's father having been appointed chaplain of the Guards' depot at nearby Caterham. This made it convenient for the lad to attend St. Dunstan's College in Catford, a technical school for senior boys, as a day student. Here he continued his studies in the physical sciences and engineering. At the suggestion of the headmaster, C. M. Stuart, a chemist who had studied under Fittig, Mees undertook a study of the preparation of o-fluorobenzalmalonic acid. Since his adviser could spare practically no time for supervision of this work, Mees was compelled to struggle by himself. As a result of this experience, and probably also of his contempt for empiricism, many years passed before he could regard the practice of organic chemistry with patience, let alone sympathy.

At St. Dunstan's Mees met S. E. Sheppard, who had entered the school at the same time. The two young men, both gifted and academically unorthodox, naturally gravitated together, and after two years, in 1900, they applied for admission to University College, London, as prospective candidates for the baccalaureate of science with a major interest in chemistry. As in the opinion of Sir William Ramsay, the professor of chemistry, they had already completed all of the routine elementary laboratory work prescribed for the degree, it was decided that they be permitted ultimately to apply for the B.Sc. by research. They were apparently the first, and probably the the last, students of the University of London to graduate under this system.

Mees, with the support of a special scholarship, was put to work under the spectroscopist E. C. C. Baly, who assigned him the task of photographing the spectrum of the iron arc with a ten-foot Rowland grating. This was his introduction to scientific photography. In order to register the lines of low frequency the plates were sensitized by bathing them in Alizarin Blue. Of his efforts in this direction Mees writes: "Looking back on the results, I can only conclude I wasn't very good at sensitizing plates, and in view of my interest ever since in optical sensitizing it's curious how bad the plates were."

Though much of his time during the first year at University College was devoted to classwork in chemistry and physics, Mees had frequent contact with Ramsay, who had completed the isolation of the "noble" gases of the atmosphere and had turned to an investigation of radium emanation. Under Ramsay's leadership the department was highly active in research in various fields of chemistry and was an ideal place for a student eager to spend all his time in the laboratory, an ambition warmly encouraged by Ramsay, who strongly advocated the granting of degrees by research.

While at St. Dunstan's Sheppard and Mees had, as a result of having adopted photographic methods of recording details of apparatus in their school and home laboratories, developed an interest in the theory of the photographic process. The books on the subject ignored the physicochemical principles recognized at that time, and the only cogent contribution relating to photography in the scientific literature was that, referred to above, published by Hurter and Driffield in 1890. With Ramsay's approval the two students embarked on a repetition of that work with improved apparatus and the application of currently available methods of physical chemistry. They constructed a sensitometer consisting of an exposure machine with a calibrated rotating sector, an acetylene standard light source, and a Hüfner spectrophotometer for the measurement of optical density. With this they measured the intensity of the images produced on development after varied exposure to light and also the rate of development under standard conditions. The results of two years of work were accepted by the University for both candidates as theses for the B.Sc. degree, granted in 1903. Mees's subsequent appraisal of the investigation is characteristic of his lifelong objectivity: "Actually, of course, the research done up to this time was of very poor quality. It was really only an orientation of the field, but the work we had done in two years was sufficient to show us that much work could be done, and we felt that by then we knew how to do it. Again, we both suffered and gained by the fact that nobody around us knew anything about the subject and that we had to work out all of it. The general opinion at University College was probably that we were extremely eccentric and rather undesirable students."

Although eager to embark on an independent career, on the advice of his father and probably also of Ramsay, he continued his researches (in collaboration with Sheppard) with the aim of ultimately graduating with a Doctor of Science degree. Ramsay's laboratories, situated in the basement under the Slade School of Art, were incapable of housing the new, more efficient, and presumably bulky apparatus required for the program. An unusual measure was accordingly adopted: the two candidates were permitted to carry on their investigations in their own homes and the only supervision they were to have was to consist of annual visits to their laboratories by Sir William Ramsay. The parents of both men fortunately possessed adequate means for the equipment of a physics laboratory for Mees and a chemical laboratory for Sheppard. Mees installed a Hüfner photometer, especially constructed for him by Adam Hilger Ltd., and a sensitometer designed and built by himself, as well as efficient, thermostatically controlled apparatus for photographic development.

For the next three years the collaboration continued in the separate laboratories, the principal subjects being: repetition and amplification of the experiments of Hurter and Driffield; the kinetics of photographic development; the structure of the developed image; the chemical nature of the latent image; extension of the color-sensitivity of photographic emulsions into regions of longer wave-length; the theory of photographic fixation. The work involved elaboration of the methods of sensitometry which thereafter remained for many years as established standards. The results, published in a series of eleven papers in the *British Journal of Photography,* were incorporated into doctoral dissertations and into a treatise entitled *Investigations on the Theory of the Photographic Process,* published in 1907 by Longmans, Green & Co.

On the completion of their academic work Sheppard and Mees were recommended by University College to the 1851 Exhibition Fellowship Board for a research scholarship. This was granted, but as only one was available decision as to the recipient was made (rumor had it) by tossing a coin. In any case, the scholarship went to Sheppard, who spent the ensuing two years in advanced study and research in the universities of Marburg and Paris. The interruption—fortunately only temporary—of the collaboration presented Mees with the problem of how to earn a living.

He had until then planned to become a schoolteacher of science, but on the advice of Ramsay he decided to enter the field of industrial research. After some fruitless attempts to secure a post in an appropriate major concern he approached his friend S. H. Wratten, the manager of Wratten and Wainwright, a small firm engaged in the manufacture of photographic dry plates. Here is his account of his entry into this firm: "Instead of a job they offered to incorporate into a company and give me a share of the business for a small sum and at the same time make me a joint managing director with Wratten. I think it was a most extraordinarily generous thing to do to a youngster who wanted a job; I have never heard of it being done by anybody else."

His first assignment was to sensitize some plates by bathing them with the new isocyanine dyes made in Germany. Having had experience in the bathing of plates he knew that this procedure gave unsatisfactory results and suggested as an alternative that the emulsion be treated prior to being coated. The steps for this procedure had to be made in total darkness, but they went well and the method gave perfect results. After some further experimentation the "Wratten Panchromatic Plates," based on similar principles, were elaborated. As they were sensitive to the entire visible spectrum they made possible the adequate photographic reproduction of paintings, provided the appropriate color filters were used. To meet these needs, the Wratten filters, consisting of gelatin films containing dyes, were developed. Permanent forms of these filters were made by cementing the films between glass plates.

Mees had a very small budget for advertising, so he put most of it into booklets containing factual information about his firm's products. These proved so valuable to customers that before long he sold them at a price which paid for their production. The principle that such information constituted the most profitable form of advertisement for technical products remained permanently a cardinal tenet of his creed.

As time went on, Mees's efforts were increasingly devoted to management of the firm's business; he nevertheless continued his researches—mainly technological—but only in the evenings and on weekends. He devised a practical wedge spectrograph, convenient for the rapid estimation of density in absorption spectra when high precision was unnecessary, and a system for the determination of photographic resolving power, thereby contributing to the techniques of three-color photography. He also investigated the potentialities of many dyes produced in Germany for their use in color filters; the results of this study were embodied in 1909 in a publication entitled *An Atlas of Absorption Spectra.*

As an outcome of the pioneer technological researches a new type of photographic plate, called "Process Panchromatic," was produced. This had a higher resolving power than the original Panchromatic plates in relation to their speed and proved of value not only in commercial photography and photoengraving but in spectroscopy and astronomy. Mees consequently formed many contacts with workers in these fields, whom he was able to help by supervising the preparation of materials specially required for scientific purposes.

At the end of two years the company had put out a long line of new products and its business had increased by over 30 percent. However, it was making practically no profit. Mees therefore studied cost accounting at night, and before long a profitable basis had been established as a result of his studies. By 1911 the business of the company had quadrupled and it was earning a net profit of 25 percent on its capital.

By now a recognized authority in industrial research, Mees became a consultant to various groups, not only in the field of photography but in others as well. In 1909 he visited the United States to assist the American Bank Note Company on problems relative to counterfeiting by photography; in 1910 he advised the Cotton Powder Company of Faversham concerning the stability of nitrocellulose; in 1911 Nobel's Explosives Company employed his consulting services in connection with cordite. At Nobel's he learned much from W. Rintoul, its research director, about the organization of industrial research. A year later he was told by Willis Whitney, the research director of the General Electric Company, that organization should be avoided as far as possible and that research men should be allowed to run their laboratories with a minimum of interference.

While in the United States in 1909 Mees visited George Eastman in Rochester and was shown the Kodak Park works. Early in 1912 he was paid a return visit by Mr. Eastman, to whom he showed the Wratten plant and his small laboratory. The next day Mr. Eastman invited him to set up and direct a research laboratory for Kodak in Rochester. This invitation was accepted on the condition, immediately agreed to, that Kodak should buy Wratten and Wainwright. The terms were arranged without difficulty, and in April of the same year Mees settled in Rochester, immediately planned a laboratory building, and began assembling a staff of leaders in the various scientific and technological disciplines. The staff included P. G. Nutting (Physics) from the U.S. Bureau of Standards; S. E. Sheppard (Physical Chemistry); A. S. McDaniel (Inorganic and Analytical Chemistry) from the Bureau of Standards; J. G. Capstaff (Photography) from Wratten and Wainwright. The first three of the divisions named were at the outset without specifically assigned projects; the photographic division was planned to undertake the development of motion picture technology and color photography. The Research Laboratory also included small manufacturing divisions for the production of photographic plates and color filters which could be marketed under the Wratten name until their sales became large enough to justify their inclusion in the list of Eastman Kodak products. The building was completed and staffed by the end of the year. At first no division of organic chemistry was set up, an omission which Mees later ascribed to his obsession with physical chemistry.

Whitney's advice with regard to the autonomy of the divisional leaders was scrupulously followed and indeed amplified. Recognizing that these men would probably be inclined to devote their chief efforts to assisting the development of technical projects, Mees counseled them not to neglect the exploration of attractive problems of purely theoretical interest that might turn up, even though such work should appear to have no direct bearing on industrial needs. This enlightened policy was based partly on his opinion that scientific specialists can keep their tools sharp only by the constant stimulation of theory and partly on his expectation, confirmed by events in the General Electric research laboratory, that by-products of "pure" research may find valuable industrial applications.

A striking example of the effectiveness of this principle occurred some years later when Kenneth Hickman, a young physical chemist from London, was assigned the task of devising a procedure for the estimation of residual solvents and moisture in photographic film base. This led into the field of manometry, and with the availability of Langmuir's diffusion pumps it branched out into studies of molecular distillation. These in turn resulted in procedures for the isolation of vitamins from natural oils now operated by the interesting subsidiary company Distillation Products Industries.

The scientific work of the laboratory was reviewed in weekly conferences presided over by Mees and attended by the divisional leaders with their associates; at these the individual sections of the work were discussed in detail by the specialists and freely criticized by their colleagues. This system provided a constantly renewed structure for the planning of future work.

During the first few years the laboratory was financed on a day-to-day basis of need, approval of expenditures being obtained from the top management of the company, and only at the end of World War I was financial stability provided by an annual budget.

In the original negotiations Mees was accorded permission to publish results of the scientific work of the laboratory, ratification to be given by Mr. Eastman personally and not by any of the other divisions of the company such as the Legal, Patent, or Sales departments. The reason he gave for this was his apprehension that "they will all be afraid that a paper they don't understand will do harm and then will say No." This system has been entirely successful. All papers have been given communication numbers, and by 1956, when Mees retired, these had reached nearly 2000. At the end of each year a volume of abridged communications was published; that for 1960 is numbered 37. In 1915 a monthly abstract bulletin was organized to cover the results of researches of photographic interest published from other laboratories. A few years later a series of six monographs on the theory of photography began to appear.

When the laboratory was built, an excellent scientific library was installed and it has since grown continually. Systematic research began early in 1913 with a staff of about twenty, working mainly on the theory of the photographic process. In 1914 Mees elaborated a method of making emulsions suitable for X-ray photography; this was first applied to the production of plates and later of films.

In 1914 he recognized the advisability of incorporating into the research organization a division of organic chemical research. After some delay this was effected, in time for the new group to undertake the experimental manufacture (starting with the nitration of benzene, the only raw material then available in the United States) of the photographic developing agents hydroquinone and p-methylaminophenol, the importation of which from Germany had been cut off by the outbreak of war. Before long the work of the organic chemists was extended to the preparation of dyes needed for experiments in color photography and the manufacture of color filters. At the end of the war the organic laboratory began to supply the company's needs for photographic sensitizers of the isocyanine and related types, one of which, previously unknown, made possible a considerable extension of sensitivity into the spectral region of long wave-length. Later, this search for new sensitizers was continued more intensively and in separate laboratories by Leslie Brooker in Kodak Park and Frances Hamer of Kodak Ltd. in England, both of whom concentrated their attention on analogous dyes derived from benzothiazole. A technique for the evaluation of these was developed by Mees, who adapted them to the production of fine-grained emulsions of high speed, which made possible the improvement of photographic materials for use in spectroscopy and astronomy. This contribution to the two sciences was recognized by the National Academy of Sciences in the award to Mees of the Henry Draper Medal in 1936.

During World War I the Signal Corps of the U.S. Army urgently needed a supply of men trained in the various techniques involved in aerial photography. To meet this demand Mees established at Kodak Park a training school, which he placed in the charge of Lieutenant A. K. Chapman, a physicist, who made so notable a success of the administration of the transient group of officers and enlisted men that when the war ended Mees retained his services as head of a new department for the development of photographic apparatus. (Chapman finally became president of the company.)

By 1918 the complete cessation of the importation of research chemicals from Germany had seriously inhibited many activities in American academic and industrial laboratories. When specific reagents were essential they had to be synthesized, often at grievous cost of time, by investigators and their associates. This situation was uniquely met in the Chemistry Department of the University of Illinois, where during the summer vacations selected students were gainfully employed in the preparation of organic compounds required for the ensuing year. This effort could of course not nearly meet the national needs, and its director, Roger Adams, in a letter to *Science* expressed the hope that some major chemical manufacturing concern would undertake the work on a large scale. As this met with no response, Mees, after consultation with George Eastman, announced the readiness of the Kodak research laboratory to attempt the task. In June 1918 the Department of Synthetic Chemistry was initiated; since male university graduates in chemistry were immediately drawn into activities recognized as essential to the war effort, the new department was at the outset staffed by girls; though enthusiastic and industrious, they seemed unable to conduct more than two preparations simultaneously and were somewhat "accident prone," so whenever they departed they were replaced by men. The initial stages of the work were facilitated by the cooperation of the Illinois laboratory, for which the Kodak group acted as distributor. Raw materials were obtained from the stocks of manufacturers and were, when necessary, purified for research purposes. During the first twelve years of operation no attempt was made to do more than meet expenses without overhead charges; at the present time the department is self-supporting and operates on a scale undreamt-of in 1918.

Another chemical offshoot of the Research Laboratory, proposed in 1926 by Mees, was the division of Cellulose Technology, which was charged with the application of organic chemical methods to cellulose. This group, which started operations with two chemists and after forty years comprises a staff of well over a hundred, has been largely responsible for the fundamental information on mixed esters of cellulose.

Among the many other technical achievements initiated in the Research Laboratory and developed under Mees's guidance was the introduction of the Cine-Kodak apparatus (1923) and the Kodachrome color film. This last supplies a typical instance of Mees's courage and readiness to proceed along unusual lines. Leopold Mannes and Leopold Godowsky (known to their irreverent juniors in the laboratory as Leo the Man and Leo the God), a pair of professional musicians, both enthusiastic amateur photographers, had invented a novel process of color photography and had consulted Mees with regard to its technical exploitation. In the belief that the new sensitizing dyes developed in the Kodak laboratories could be of value to the proposed procedure, Mees invited the inventors to continue their experiments in cooperation with his emulsion research group. This collaboration was eminently successful; the resulting Kodachrome film (16 mm.) was placed on the market in 1935, together with a system for making color prints on white support and a negative-positive process termed Kodacolor.

In 1923 Mees was appointed director of Eastman Kodak Company and in 1934 he became vice president in charge of research and development. In 1956 he retired from this post and took up permanent residence in Honolulu. At their farewell session the Board of Directors recorded the following tribute: "Many others have done him great honor which we cannot enhance, but we alone can give our personal testimony of appreciation and gratitude for the inspiration of his genius and leadership in Kodak affairs, and for the heritage of high aims and great accomplishment which he has left to us."

Mees was an excellent lecturer and public speaker on scientific, technical, and philosophical subjects, being both gifted and careful in the presentation of well-established facts. In conversation, on the other hand, he frequently gave the impression of being willing to overstate a thesis for the purpose of startling his hearers into unexpected opinions. During his first years in Rochester some of his new colleagues found this unduly positive mode of expression hard to take; this reaction was, however, gradually overcome, for the breadth of his information and the general soundness of his judgment counteracted the antipathy which his manner aroused, and ultimately his advice was freely sought by his associates.

In 1943 he accepted an invitation from the University of California to give a series of lectures as Hitchcock Professor. The topics embraced in these lectures, relative to the development of science and its relationship to the history of society, were incorporated in a book entitled *The Path of Science,* published in 1946 by John Wiley and Sons, Inc. He was accorded two honorary Doctor of Science degrees: by the University of Rochester in 1921 and by Alfred University in 1950. Having retained his British nationality, in 1939 he was elected to fellowship in the Royal Society. In 1950 he was made a Fellow of University College, London, and having by then acquired American citizenship he was elected a member of the National Academy of Sciences.

During his student years, Mees's life in London opened his eyes to the prevalence of extreme poverty, and when he learned that one third of the population of the city consisted of members of families whose weekly income was less than one pound he became a Socialist and joined the Fabian Society. There he met what in his memoirs he described as "a group of intellectuals who were trying to organize things so that the socialists could take over the government of England." However, after many years he recognized that "when socialism was tried, it did not succeed" and that "there are better ways of dealing with poverty than the socialist way." He also came around to the view that whereas "the important thing was the acquisition of knowledge, perhaps the next most important thing was the application of the knowledge to industry" and that it would be "much better to increase the total amount of wealth than it was to redistribute it."

Another youthful tenet which he ultimately abandoned as a result of more accurate information and more mature judgment was that vaccination could give no protection against smallpox.

In his late thirties he was willing to admit, though only to close friends, the loss of his ability to subscribe to the doctrines of any organized religion. This change of view, being a purely personal matter, was never openly proclaimed, particularly as he clearly wished to avoid giving pain to his gentle and devout father. His freedom from racial prejudice and his intrinsic kindness were shown by his response to a remark containing the expression "Wop": "Oh, you mean a compatriot of Galileo and Dante?"

In 1909 he married Alice Crisp, with whom he shared forty-five years of happy married life. They had two children: Graham (1910) and Doris (1912). In 1951 he lost one of his legs by amputation as a result of a massive thrombosis. In his remaining years he used an artificial limb and even managed to drive his car. From Honolulu he wrote in a personal letter: "I considered building a little laboratory here and doing some experimental work, but my inability to walk prevented it. I am sure that while I should build the laboratory with enthusiasm, I should never use it. I can't stand well enough, and it isn't easy to do lab work without standing. Anyway, I find plenty of occupation. I see lots of people and am generally having a very good time."

His death, in Honolulu in 1960, came painlessly and without warning.

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KEY TO ABBREVIATIONS

Astrophys. J. = Astrophysical Journal
Brit. J. Phot. = British Journal of Photography
Colour Phot. = Colour Photography (supplement to British Journal of Photography)
Ind. Eng. Chem. = Industrial and Engineering Chemistry (previously Journal of Industrial and Engineering Chemistry)
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Franklin Inst. = Journal of the Franklin Institute
J. Opt. Soc. Am. = Journal of the Optical Society of America
J. Roy. Soc. Arts = Journal of the Royal Society of Arts
J. Soc. Chem. Ind. = Journal of the Society of Chemical Industry
J. Soc. Motion Picture Television Engrs. = Journal of the Society of Motion Picture and Television Engineers (formerly Transactions of the Society of Motion Picture Engineers, then Journal of the Society of Motion Picture Engineers)
Mech. Eng. = Mechanical Engineering
Phot. J. = Photographic Journal
Proc. Roy. Soc. London = Proceedings of the Royal Society of London
Sci. Am. = Scientific American
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Seth B. Nicholson

SETH BARNES NICHOLSON

November 12, 1891–July 2, 1963

BY PAUL HERGET

SETH BARNES NICHOLSON was most distinctly recognized in astronomical circles for his discovery of four satellites of Jupiter, a feat equaled only by Galileo when he directed his first telescope toward the planet. Nicholson was wont to quip that all that was needed to discover a new satellite was the largest telescope in the world. These discoveries were surrounded by interesting sidelights, and they formed part of a significant series of events having important bearing on theories of the origin of the solar system.

Using the 36-inch Crossley reflector of the Lick Observatory, C. D. Perrine discovered, in December 1904, a faint satellite of Jupiter, moving in an orbit 7.5 million miles from the planet, in a period of 260 days. In 1905 he discovered another satellite, and for the first time in the history of astronomy the orbits of two satellites were found to be at the same distance from their primary. Yet these orbits were interlocked and linked in such a way that the two objects could not collide under present circumstances. At the Greenwich Observatory in 1908, Melotte discovered another outer satellite, twice as distant from Jupiter, and having a period slightly over two years. This, for the first time, revealed a satellite of Jupiter revolving in the opposite direction from all the other satellites, and from the rotation of the primary.

Nicholson entered this scene in 1914, when there was an eclipse of the Sun visible in Russia. The senior astronomers of the Lick Observatory went to observe the eclipse, and the director left orders behind that this faint, new, retrograde satellite was to be observed whenever telescope time was available. The assignment fell to Nicholson; as an astute young graduate student, he took an extra long exposure in order to be on the safe side. The result was that he discovered yet another satellite in the vicinity, and it proved to be a "twin" of Jup. VIII, also having retrograde motion, the same period and distance, and again being interlocked in such a way as to preclude collisions. This was no mean feat of observing, since the object was barely on the limit of detectability of the 36-inch telescope and the photographic plates then in existence. The whole experience presaged not only Nicholson's ability as a keen and ardent observer but also his ability to handle the theoretical side of the astronomical problems he encountered. The discovery provided the material for his Ph.D thesis. His computation of the orbital elements and ephemerides typified the first-rate treatment he brought to bear on all his work.

For over two decades he was the target of occasional jibes from his Mt. Wilson colleagues to the effect that, since he was the only one who had ever observed this satellite, it could very well be a hoax which he had perpetrated. Then in 1938 the International Astronomical Union meeting in Stockholm attracted many of the Mt. Wilson astronomers so that enough observing time on the 100-inch telescope became available to Nicholson to enable him to conduct a search over some $6^\circ \times 2\overset{\circ}{.}5$ of the sky for more satellites in the vicinity of Jupiter. This led to the discovery of more than forty faint, moving objects, each of which must patiently be observed and followed until it could definitely be established as a foreground object moving in an orbit about the Sun. The result led him not only to the independent rediscovery of each of the known satellites but also to the discovery of two more. Each one matched one of the known pairs, so that now each became a set of "triplets," a circumstance which has no known counterpart anywhere in the solar system. The following year I saw Nicholson's observation book with notations indicating Jup. XII and Jup. XIII. But these only proved to be the most recalcitrant cases, which eventually joined all the others which had to be rejected. When asked if he got a thrill out of his satellite discoveries he indicated that he did not, because the "discoveries" are so slow in coming and because it takes so long before one can be certain about them. In 1951, while intending to observe Jup. X, he discovered Jup. XII. It also has a retrograde orbit, but it does not fit quite so closely the elements of Jup. VIII, Jup. IX, and Jup. XI. Nicholson was finally of the opinion that there are still fainter satellites of Jupiter to be discovered. I often wondered why he never made a similar survey around Saturn, but I never had the chance to ask him.

Seth Nicholson's forebears can be traced back through several lines of Scottish and English ancestry. Martha Ames, his mother, was of the eighth generation from William Ames, who emigrated from England in 1635. All these Ameses lived in the vicinity of Boston, until her father moved to Iowa. Another maternal line can be traced to John Cary, who joined the Plymouth Colony at about the same time as William Ames's arrival. His name appears in the land deed granted by Massasoit "to Miles Standish . . . in behalf of William Bradford, John Cary, . . ."(and 52 others who are named). Seth's great-grandmother was descended from Rev. James Keith who came to Boston from Aberdeen, Scotland, in 1662. An uncle and his maternal grandfather were both ministers of the Disciples of Christ church. In 1885 the grandfather, Rev. Lucius Bowles Ames, moved from Toulon, Illinois, to Des Moines, Iowa, so that his children might attend Drake University.

Seth's paternal grandfather was a pioneer farmer who emigrated from Scotland to Illinois. William F. Nicholson, Seth's father, graduated from Knox College in Galesburg, Illinois, and then earned a master's degree in geology at Cornell University. He became an elementary-school teacher and principal at Springfield, Illinois. He married Martha Ames on August 21, 1888. Seth Barnes was born November 12, 1891. An older sister, Neva, devoted her active life as a missionary in India. Two younger sisters, Carrie and Helen, served as schoolteachers in Pasadena and Los Angeles. In 1898 William Nicholson moved his family to a farm near Toulon, Illinois, for reasons of his health. After six years he returned to teaching, and became the principal of the Toulon Academy (a high school). Thus it happened that Seth spent his first grade and all of his high school years in schools where his father was the principal. He attended a country school for the remainder of his elementary school years.

In this rural environment, his youth included all the experiences of a farm hand, and the summers of his high school years were spent in such employment. But the intellectural atmosphere of his home was not lost upon him, and even as a boy he owned a small telescope and watched the stars. He once wrote to an inquiring young correspondent: "When I was a boy our family lived on a farm in northern Illinois. My father had been a geologist and a schoolteacher before we moved to the farm because of his health. He was interested in science of all kinds and from him I learned the names of the stars, flowers, birds and rocks. I was then just as interested in geology and botany as in astronomy. Electricity was my favorite hobby and my father showed me how to make toy motors, telegraph instruments, and induction coils.

"I remember watching an eclipse of the sun when I was a boy, but I was not especially interested in astronomy before going to college at Drake University."

Nicholson enrolled at Drake in 1908, and the turn which his career took was shaped by Dr. D. W. Morehouse, the Professor of Astronomy. Morehouse had just discovered a comet which reached a splendid naked-eye visibility, and two years later Halley's Comet appeared. Some of the classic photographs of Halley's Comet were taken by Nicholson with the 8-inch telescope at Drake, even though he was still an under graduate. We have no sure way to assess, we can only surmise from the external evidence, the influence of one of his classmates, Miss Alma Stotts. They worked together to compute the orbit of a minor planet which had been discovered by Metcalf in the winter of 1909, and then prevailed upon Metcalf to name it Ekard, or Drake spelled backwards, in honor of their school. After graduation from Drake in 1912, they both enrolled in the graduate department of astronomy at the University of California at Berkeley. Their names appear as co-workers in the computation of several comet orbits. They were married on May 29, 1913. Their home was blessed with three children, Margaret (1915), Donald (1918), and Jean (1921).

A perusal of Nicholson's bibliography of 267 entries reveals at the beginning his interest in the small bodies of the solar system: minor planets, comets, and satellites. This interest never waned. In 1936 he extended the observed arc of Adonis by a whole month; and after his retirement he discovered another Trojan minor planet . In 1930 when Pluto was discovered, Nicholson was the forerunner in the competition to derive a reliable solution for its orbit. He found the four observations on Mt. Wilson plates which had been taken in 1919. His experience and judgment enabled him to ferret out an erroneous prediscovery observation, and he was the first to demonstrate that the determination of the mass of Pluto depended almost entirely upon the less precise 1795 observation of Neptune.

Nonetheless, after receiving his doctorate at Berkeley he was appointed to the staff at Mt. Wilson, which was then primarily a solar observatory, and so most of his career was spent in observational work on the Sun. Beginning in 1920 he issued annual reports on sunspot activity and additionally since 1940 there were reports on solar and magnetic data. The continuous solar history over several sunspot cycles, as recorded by the tower telescopes at Mt. Wilson, is in the largest part Nicholson's work during those years.

In the early 1920s he collaborated with Dr. C. E. St. John to establish an independent value of the solar parallax from extensive spectroscopic observations of Venus. A significant series of scientific contributions came from his collaboration with Edison Pettit in making vacuum thermocouple measurements of the radiation and temperatures of the planets and the Moon. Thus, for the first time, conjecture was replaced by evidence. The rapid fall and rise of the lunar temperature during an eclipse from 350°K to 150°K and back was observed. Likewise the 600°K temperature of Mercury, the relatively lower temperature of sunspots, and the low density of the atmosphere of Mars were all established. The delicacy of these observations in those early years is hard to realize. The sensitivity required with the equipment used has been compared to detecting a candle light 100 miles away.

Nicholson was an observer with solar eclipse expeditions in 1923, 1925, 1930, and 1932. During World War I he taught navigation, and during World War II he was engaged in several classified projects which included investigations of aerial cameras, as well as work at Los Alamos. From 1946 to 1961 he collaborated with O. R. Wulf in detailed studies of the relationship of geomagnetic and solar activity.

Another facet of Nicholson's career was his hearty participation in the astronomical community of the West Coast. He served the Astronomical Society of the Pacific as president in 1935 and 1960, and as editor of its *Publications* from 1940 to 1955. On numerous occasions, as Morrison Lecturer or as a visiting lecturer of the Society, he addressed local sections or school groups. He had a tremendously appealing platform personality when lecturing, and he was equally at ease whether with astronomers or with students. After retirement, he was coordinator of the western region for the visiting professors program of the American Astronomical Society and the National Science Foundation and presented 45 lectures himself in visits to the central and western states during 1957-1962. This quotation from a local newspaper (1930) illustrates the reception of his lectures.

"Dr. Seth B. Nicholson lectured before a capacity audience at Cal. Tech. in one of a series of talks conducted by the Astronomical Society of the Pacific and the local observatory.

"The audience agreed the lecture was one of the best ever heard during the history of the astronomical series; that Dr. Nicholson possesses the unusual ability to transmit to the layman the results of his profound studies."

Excerpts from a letter written by one of Nicholson's sisters illustrate his experiences. "In 1962 he was asked to give a series of talks at the California Christian Home for Senior Citizens. He gave three talks which thrilled the people there. He spoke of the good questions that some of them asked at the close of the lecture. . . . A 97-year old lady in the Scripps Home asked him to come there to lecture as the main feature of her birthday party. He delighted in every indication that people should continue to learn as long as they lived."

Seth was fond of recreation. He enjoyed a vigorous game of tennis, and often omitted lunch in order to play ping-pong with members of the Observatory staff. With scouts or his family, he was an inveterate hiker on the mountain trails above Altadena.

Nicholson served in numerous civic and community enterprises, and was an especially beloved scoutmaster from 1923 to 1938. Afterward he was troop committeeman and later commissioner. He received the Silver Beaver Award as a tribute to his outstanding scouting service. From 1950 to 1962 he served on the Altadena Library Board successively as trustee, secretary, and president.

It was a delight to know Nicholson and to be his friend. He was helpful to everyone, but always in a sensible way. As R. S. Richardson relates, Seth provided some technical information to a movie studio, but in return he insisted on six free passes to visit the movie set during filming—not to be used for himself, but for some likely visitor. When my wife and I visited Pasadena the first time, we had barely introduced ourselves before he had arranged for our visit to the Huntington Memorial Library. He was like that. The answering of written inquiries to the Observatory just naturally seemed to gravitate to him. One graduate student who was working on a doctoral research project wrote, in part, "If you are willing, please briefly comment on your interpretation of the scientific method or scientific methods." Nicholson replied:

"During my career as an astronomer I have given very little thought to the 'scientific method.' Curiosity about the unknown kept me going. That and the fun I've had finding out about things and telling others about them, especially young people.

"I have been primarily interested in observing. It has never seemed necessary to have a theory to be proved or disproved by my observations. The temperatures of the moon, planets, sun, and stars were measured to find out what they were, not to prove some theory about life on the planets or about stellar evolution. The calculation of a comet's orbit satisfied my curiosity and I enjoyed filling pages with figures to learn about the orbit, not to test a theory about the origin of comets.

"It has seemed to me that good observations were the basis of science. Scientific observations must be checked and controlled in systematic ways. My 'scientific method' has been 'check and double check.' I know that observations alone do not tell the whole story, but I have been content to let others make up theories to fit the observations and was glad when they did. The new and most interesting observations are seldom directed by theoretical predictions.

"Several articles about the 'scientific method' have appeared in the *American Scientist.* I like 'To Tell or Hear Some New Thing,' by Joel H. Hildebrand in the March 1963 number. If you have read it, I do not need to say more. If you have not, you should."

A few of the many tributes sent to Mrs. Nicholson by younger astronomers are revealing.

"I give credit to Seth for getting me started in the right way on my professional career. It was he who told me of an opening for a computer at Mt. Wilson. That opportunity opened a new world to me, for which I have Seth to thank. Also while in Pasadena those two years I had the privilege of working with Seth on the orbit of Pluto. That experience is unforgettable: under his guidance and stimulation, I learned thoroughness, accuracy, and imaginative approach to orbit work. It was apparent to me then, and was in subsequent years re-enforced, that Seth was one of the best, most broadminded, and expertly informed astronomers on the Mt. Wilson staff. In addition to his professional competence, Seth was outstanding for his handling of young people with great good humor and sound counsel. I was indeed fortunate to have been associated with him so early in my career in astronomy."

"It is difficult to write what you already know—that Dr. Nicholson was effectively a father to the entire astronomical community, and that we all share your missing him. For the last 10 to 12 years, I am sure I have asked him for advice about twice a year—and I owe a lot to the fact that I followed it. I realize now that I probably never specifically told him how much this meant to me, and even when I did *not* rely on him, it was nice to know that he was 'available.' The home you have with him must be wonderful."

Nicholson held memberships in the American Astronomical Society, Astronomical Society of the Pacific, American Association for the Advancement of Science (Chairman of Section D, Astronomy, 1944), International Astronomical Union, the National Academy of Sciences (1937), Phi Beta Kappa, and Sigma Xi. He received an honorary LL.D. degree from Drake University in 1949. Undoubtedly the honor that he treasured most highly was the Catherine Bruce Gold Medal, awarded by the Astronomical Society of the Pacific on June 13, 1963. Tragically, he had to hear the presentation ceremonies over a telephone hook-up from the San Diego meeting to his hospital bed in Los Angeles. He died shortly afterward on July 2.

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KEY TO ABBREVIATIONS

Astron. J. = Astronomical Journal
Astron. Soc. Pac. Leaflet = Astronomical Society of the Pacific Leaflet
Astrophys. J. = Astrophysical Journal
Carnegie Inst. Wash. Publ. = Carnegie Institution of Washington Publication
Griffith Obsr. = Griffith Observer
J. Geophys. Res. = Journal of Geophysical Research
J. Opt. Soc. Am. = Journal of the Optical Society of America
Lick Obs. Bull. = Lick Observatory Bulletins
Mt. Wilson Commun. = Mount Wilson Communications
Mt. Wilson Contrib. = Mount Wilson Contributions
Mt. Wilson Palomar Obs. Reprint = Mount Wilson and Palomar Observatory Reprint
Phys. Rev. = Physical Review
Pop. Astron. = Popular Astronomy
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Publ. Am. Astron. Soc. = Publications of the American Astronomical Society
Publ. Astron. Soc. Pac. = Publications of the Astronomical Society of the Pacific
Terr. Mag. = Journal of Terrestrial Magnetism and Atmospheric Electricity
Trans. Am. Geophys. Union = Transactions of the American Geophysical Union
Trans. Internat. Astron. Union = Transactions of the International Astronomical Union

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Gregory Pincus

GREGORY GOODWIN PINCUS

April 9, 1903–August 22, 1967

BY DWIGHT J. INGLE

GREGORY PINCUS achieved important aims in life and had a salubrious influence upon the lives of many others and he died while fully active as a scientist-statesman of the world. Gregory Pincus was an ambitious man who sought, gained, and enjoyed power in certain fields of biology which relate to medicine. He did not achieve power and fame by great discovery, luck, privilege, or favoritism but by devotion to noble goals in which he wholeheartedly believed. He could have used his knowledge and influence to gain wealth but did not. Among a host of individual achievements, three especially brought the power that he quietly cherished. These were the organization and direction of the Laurentian Hormone Conference, the organization and direction, with his friend Hudson Hoagland, of the Worcester Foundation for Experimental Biology, and the development of the antifertility pill.

He was interested in the processes of reproduction and aging, and in several of the great diseases, such as arthritis, cancer, and psychoses. He was interested in the functions of the adrenal cortex and its response to stress. He took the chair at many conferences. He served as chairman of the Endocrinology Study Section of the United States Public Health Service and as chairman of the Endocrinology Panel, Cancer Chemotherapy National Service Center, of the National Institutes of Health. It was claimed by some that he held too much power over other endocrinologists but I was there and remember him only as a skillful, efficient, impartial chairman. He wrote several books and monographs.

Among the honors received by Gregory Pincus were the Oliver Bird Prize (1957), the Albert D. Lasker Award in Planned Parenthood (1960), the Sixth Annual Julius A. Koch Award (1962), the *Modern Medicine* Award for Distinguished Achievement (1964), the City of Hope National Medical Center Award (1964), and an honorary professorship, San Marcos University, Lima, Peru. He became a Fellow of the American Academy of Arts and Sciences (1939) and a member of the National Academy of Sciences (1965); he was President of the Endocrine Society (1951-1952); he received the Cameron Prize in Practical Therapeutics from the University of Edinburgh (1966) and the Scientific Achievement Award of the American Medical Association (1967).

He was born in Woodbine, New Jersey, the eldest son of Joseph W. and Elizabeth F. Pincus. The father was a graduate of Storrs Agricultural College in Connecticut, taught at a school of agriculture, and edited a farm journal. The mother came of a family which was distinguished in the agricultural sciences. Gregory Pincus was born into an environment which stimulated interest in books, culture, and science.

He attended elementary and secondary public schools in New York City and became an honor student at Morris High School, from which he graduated. He was president of high school literary and debating societies. He earned money doing odd jobs outside of school and doing farm work during summers.

In 1924 he received a B.S. degree from Cornell University. While there as an undergraduate he founded and edited the *Cornell Literary Review*. He entered graduate school at Harvard, majoring in genetics and minoring in physiology. He received the M.S. and Sc.D. degrees in 1927. Following this, he became a National Research Council Fellow for three years and studied at the Kaiser Wilhelm Institute and at Cambridge University. He returned to Harvard as an instructor in biology in 1930 and became an assistant professor in 1931, holding this appointment until 1938. He spent 1937-1938 as a visiting investigator at Cambridge University.

His early training and research represented an interest in the inheritance of physiological traits. His first scientific paper, published in 1926 with Horace W. Feldman as co-author, was "On the Inheritance of Albinism and Brown Pigmentation in Mice." Other studies on genetics were done later. A series of studies of geotropisms in rats and other mammals in which the mode of transmission of proprioceptive function was analyzed were reported by Gregory Pincus and Professor W. J. Crozier of the Biological Laboratories of Harvard. There were related studies of the transmission of reaction mechanisms governing respiratory movements and heartbeat in inbred mouse strains. He was influenced by Crozier and by W. E. Castle to turn to reproductive physiology and he published a paper on the living eggs of the rabbit in 1930. During the early part of the 1930s his interest in reproductive physiology, including the sex hormones and the gonadotrophic hormones, was represented by research reports in this field, but he also did collaborative studies on the inheritance of diabetes mellitus with members of the staff of the famed Joslin Clinic in Boston, especially with Priscilla White. With R. Shapiro, he developed the first successful method of extensive partial pancreatectomy in the rat, a procedure that has been widely used.

In 1932, with O. S. Baum as co-author, Gregory Pincus published his first paper on ovarian hormones and the gonadotrophic principles of pregnancy urine, and for the rest of his life much of his research was focused on the hormones associated with reproductive processes. He studied living ova *in vivo* and *in vitro* and the processes of fertilization. During these years at Harvard, he pioneered in producing multiple ovulation in animals and in transplanting animal ova from one female into another which then carried growing embryos to term. He found that certain phases of ovum development *in vivo* were regulated by ovarian hormones and launched a series of studies on ovarian hormone biogenesis and metabolism along with studies of their effects on ovum travel, uterine function, and blastocyst and embryo maintenance. Studies on the actions, metabolism, and biochemical determination of ovarian hormones grew apace during these years. Toward the end of the 1930s he published reports on the production of fatherless rabbits produced by the artificial activation of ova.

Suddenly, Gregory Pincus became widely known, for the news appealed to the lay press and the public. His friends began to refer to the process as "Pincogenesis." The achievement has never been shown to be independently reproducible upon demand, a requirement for proof of a claim in science. His detractors, of whom there have been a significant number, have never forgotten the matter; his friends and admirers, not all free of uncertainty about the validity of the claim, believe that he was always an honest scientist who did not knowingly go beyond his facts.

In 1938 Gregory Pincus moved from Harvard to Clark University where he became a visiting professor of experimental zoology until 1945 and held a Guggenheim fellowship from 1939 to 1941. He also held a titular professorship in physiology at Tufts Medical School from 1946 to 1950, which was followed by a similar professorship in biology at Boston University Graduate School.

During the spring of 1943, a conference on hormones, sponsored by the American Association for the Advancement of Science, was held at Gibson Island near Baltimore. It was a significant event in the lives of some thirty participants, especially for Gregory Pincus, and the consequences had an enduring impact throughout the world of endocrinology. The conference was organized by Hans Jensen, then of the Upjohn Company, and by Fred Koch of the University of Chicago. The conference was a great success but for many reasons Gibson Island, a private club, was an unpleasant place to meet. For example, a distinguished Negro scientist, Dr. Percy Julian, had been invited to the conference. He was not permitted to enter the club until after three days of protesting by members of the conference. Significantly, it was Gregory Pincus who led the belatedly successful appeal to the management of the club to permit Dr. Julian to join us. The group wanted to meet again but not at Gibson Island. Gregory Pincus, Robert W. Bates, and Samuel Gurin were selected to arrange the next conference. The Montreal Physiological Society invited the group to come to Canada and hold the conference at Mont Tremblant. And so the Laurentian Hormone Conference was born with Gregory Pincus as the permanent chairman.

The Laurentian Hormone Conference was a success from the beginning but determination and skill in human relations were necessary to carry it past threatening problems. The personal qualities of Gregory Pincus became known and appreciated by his fellow conferees. He was not a modest man but neither was he haughty nor was he intolerant of the limitations and foibles of others. It was not possible to publish the papers presented at the first hormone conference as planned but an annual volume called *Recent Progress in Hormone Research* was published each subsequent year and the publication lag was reduced to a reasonable period. Early financial problems were solved in part by contributions from pharmaceutical houses. During the second annual meeting at Tremblant, the anti-semitism of the owner of the club became overt and arrangements were made to meet elsewhere. The Laurentian Hormone Conference did not return to Tremblant until the owner of the club died and it came under new management.

Membership in the conference required application and it was not possible to accommodate all applicants. For a time, many who were not invited to the conference became critical of what they claimed were arbitrary decisions by Gregory Pincus to invite only his personal friends. These criticisms were not deserved. Gregory was never overtly distressed by criticism or hostility but those close to him knew that he was hurt by accusations of partiality. He established an anonymous committee which processed applications for membership and assigned priority ratings favoring qualified endocrinologists who had not been accommodated the first or second year of application. He appointed committees on arrangements and on programs as well as a senior advisory committee of some of us who were "old guard." Criticisms of the management of the conference by Gregory Pincus gradually abated, and his skills as an organizer became widely appreciated. When the business affairs of the conference grew, a corporation was formed to handle its funds and business transactions. After the conference began to earn its way, it became possible to pay the traveling expenses of invited speakers from abroad. It was characteristic of Gregory Pincus to be unobtrusive; he spoke briefly to open each conference and to set the tone of informality and good humor. Thereafter he made necessary announcements, but generally kept in the background.

This, in brief, is how Gregory Pincus came to create a ferment in the field of endocrinology, to facilitate the exchange of ideas and the excitement of discovery, and to winnow out much of the dogma which was once taught about the ductless glands. The Laurentian Hormone Conference set new standards in a field previously dominated by physicians who were strangers to the laboratory by bringing together from all over the world outstanding medical and nonmedical scientists from universities, institutes, research hospitals, and from industry.

Although Gregory Pincus did not achieve success by falling heir to a kingdom, there were several friends and admirers who helped him create domains, and to them he was deeply grateful. The most important was his friend Hudson Hoagland, with whom he joined in 1944 in founding the Worcester Foundation for Experimental Biology (WFEB). This was a bootstrap operation representing scientific free enterprise, and under the co-directorship of the two men it grew and attracted to it some of the world's most gifted young endocrinologists. Pincus now assumed the role of a research director. Although active in the planning of research, he did less and less of it with his own hands. He was now creating a role for himself as an organizer and director and was to develop into a scientist-statesman.

One of the most remarkable and fruitful accomplishments at the WFEB was the perfusion of beef adrenal glands so that large amounts of corticosteroids were produced. Gregory Pincus believed the procedure had commercial possibilities. He assigned Oscar Hechter to the problem. Five years passed before beginning success was achieved, much of the credit for which belongs to Hechter. By then the chemical synthesis of the corticosteroids had become successful and microbiological methods were soon to come. Hechter was among the first to predict the usefulness of the latter. He and his team of collaborators, whose selection and organization were guided by Pincus, then exploited this biological system to elucidate the steps of biosynthesis of the corticosteroids and the site of action of the adrenocorticotrophic hormone of the anterior pituitary.

The WFEB became an important center for research on the steroid hormones and for training young investigators in the methods of steroid biochemistry. It represented many other research activities as well. Hoagland and Pincus invited to it many gifted young scientists who were in need of opportunity. The two men, Hoagland, the Executive Director, and Pincus, the Scientific Director, so different in many traits of personality, were alike in exhibiting a generous spirit.

His titular professorships, first at Tufts and later at Boston

University Graduate School, were for the purpose of allowing graduate students to work for the Ph.D. at the WFEB. Although Gregory Pincus was a gifted lecturer, he devoted relatively little time to formal teaching. Most of the young scientists whose advancement was enhanced by him were postdoctorates and were already zealous achievers by the time they came to him.

It is important to mention Min Chueh Chang, one of the first members of the Institute, who became a foremost authority on mammalian reproduction. Studies done in the early 1930s had demonstrated that the overdosing of laboratory animals with ovarian hormones would interfere with the reproductive cycle and cause sterility. In 1936, Pincus and Kirsch published on sterility in rabbits caused by injections of oestrone and related compounds. Subsequent studies demonstrated the possibility of achieving physiologic control over fertility rather than pathologic interference with reproduction. M. C. Chang began to search among the steroids for antifertility agents and found a number which were orally active in rats and rabbits. Gregory Pincus, influenced by his friend Margaret Sanger, knew the need of the world for effective social control of human fertility and had the knowledge and the courage to aim for this goal.

Funds for research on fertility-controlling agents could not be obtained fifteen years ago from the large foundations or from the federal government. However, the late Mrs. Stanley McCormack generously contributed a sizable annual budget to support this work and did so continually until her death in January 1968. Without her aid, this program of research would have been greatly handicapped. Later, the Josiah Macy, Jr. Foundation and the G. D. Searle Company, and still later the Ford Foundation, through the Population Council and the National Institutes of Health, contributed to this work.

When the clinical testing of antifertility agents by Dr. Celso Garcia and Dr. John Rock confirmed the results on experimental animals, Gregory Pincus set up clinical field tests in Puerto Rico and Haiti. He now became an entrepreneur and a missionary who traveled all over the world studying the problems of human numbers, teaching means of control, and recruiting disciples. Always a voracious reader, he kept in touch with the scientific literature and with the details of research in his Institute.

Gregory Pincus was sensitive to the medical, ethical, political, and religious problems relating to population control and he responded to them as a wise scientist. At least twice in his life he had authored research reports in which the conclusions were not documented by well-controlled experiments. He knew that he had been criticized and was now determined that each step forward should be supported by unassailable evidence. The long-term effects of antifertility agents are not yet known with certainty and now and then there are sobering claims for pathogenesis in a small number of patients, but in general, the laboratory and clinical testing of these agents was done in the best possible way.

The activities described above, plus love and selflessness to family, would have drained the energies of any man not well endowed with physical strength and emotional reserve, but Gregory Pincus attended to these and many other interests and responsibilities, and for most of his life seemed as fresh and relaxed as he was urbane. When confronted by emotional flare-ups, he remained calm. He knew how to relax. I am reminded again of the first hormone conference held at Gibson Island. A number of conferees were crowded into an untidy, too warm, poorly ventilated dormitory where the odds against restful sleep were higher than wanted. It was Gregory Pincus who slept soundly and soundfully throughout the night and awakened in the morning to remark to his less fortunate companions, "Isn't it amazing that this number slept in one room and nobody snored?"

I do not know when Gregory Pincus became ill, but during the last three years of his life there were times that he looked unwell and there were rumors that this was so. He either did not then know that his illness was serious or, if he recognized the fact within the quiet of his mind, he did not want it known to others. I quote from a letter written by him to me March 28, 1966: "Oscar Hechter tells me that you have been informed that I am a seriously ill man. This is entirely untrue, and I wish you would do your utmost to scotch this rumor. At the moment I am healthier than I have been in many years. As nearly as I can make out, a minor setback which occurred about a year ago has been the source of the unfounded rumor." Shortly thereafter, a physician friend told me the nature of Gregory's illness, a correct deduction which was made from his appearance and a knowledge of the symptoms rather than a personal medical examination. In any case, Gregory Pincus would not play the role of an invalid. When it became known that he had myeloid metaplasia, he assured his friends that modern therapeutic measures would allow him to live into retirement years. He continued to work and travel. I received a letter from him written a week before he died, asking me to chair a session at the 1967 Laurentian Hormone Conference. I was traveling on the day of his death and did not learn of it until we arrived at Tremblant. His secretary said that in lucid moments prior to death he asked that the hormone conference be held as planned. I remembered that many years earlier Gregory Pincus had opened a conference with the announcement of the first deaths among members of the old guard and had commented that the future of the hormone conference was uncertain, that man is mortal, and that the conference might not endure beyond the lives of those who presently organized and enjoyed it. But when Gregory Pincus died, the Laurentian Hormone Conference had long since become a viable, healthy institution, with an organization and gist that would serve the future as a living memorial to the man who guided it for more than two decades.

Oscar Hechter has written a homage* to Gregory Pincus in which he says, "I feel that Gregory Goodwin Pincus—Goody to his friends—is too big a man to treat in a ritualistic fashion. Pincus for me represents the prototype of a *new* scientist, whose life and achievements merit critical examination and analysis. On a planet rapidly being irreversibly transformed by science and technology in ways not clearly foreseen, we desperately need information about the mechanisms by which individual scientists change the world. Pincus and his life merit a critical case history, because if new Pincuses arise in the future, they will have a powerful impact upon the world."

Hechter examines the life of Gregory Pincus candidly and concludes that he was a great man because he was a person of action who showed the world that the population crisis is not an impossible problem. Few scientists have had so great an impact upon the lives of others as did Gregory Pincus by promoting the antifertility properties of certain steroids, now known as The Pill. The Pill may not remain the antifertility agent of choice, but its development created a climate of interest and opinion needed for extensive research on antifertility and the aim to apply this knowledge to save mankind.

To oversimplify, some scientists become great by making important contributions to knowledge—discovery in the laboratory—and others become great as organizers and by making important applications of knowledge. Gregory Pincus, a scientist-statesman, was one of the latter.

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* Oscar Hechter, "Homage to Gregory Pincus," *Perspectives in Biology and Medicine,* Spring 1968 (an *In Memoriam* issue to Gregory Pincus).

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KEY TO ABBREVIATIONS

Acta Endocrinol. = Acta Endocrinologica
Acta Endocrinol., Suppl. = Acta Endocrinologica, Supplementum
Am. J. Med. Sci. = American Journal of Medical Sciences
Am. J. Obstet. Gynecol. = American Journal of Obstetrics and Gynecology
Am. J. Physiol. = American Journal of Physiology
Am. J. Psychiat. = American Journal of Psychiatry
Am. Naturalist = American Naturalist
Am. Zool. = American Zoologist
Anat. Record = Anatomical Record
Anat. Record Suppl. = Anatomical Record Supplement
Ann. N.Y. Acad. Sci. = Annals of the New York Academy of Sciences
Arch. Biochem. = Archives of Biochemistry
Arch. Biochem. Biophys. = Archives of Biochemistry and Biophysics
Biochem. Pharmacol. = Biochemical Pharmacology
Bull. Post Grad. Comm. Med. Univ. Sydney = Bulletin of the Postgraduate Committee in Medicine, University of Sydney
Cancer Res. = Cancer Research
Ciba Found. Colloq. Aging = Ciba Foundation Colloquia on Aging
Ciba Found. Colloq. Endocrinol. = Ciba Foundation Colloquia on Endocrinology
Current Med. Dig. = Current Medical Digest
Federation Proc. = Federation Proceedings
Gen. Comp. Endocrinol. = General and Comparative Endocrinology
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Aviation Med. = Journal of Aviation Medicine
J. Biol. Chem. = Journal of Biological Chemistry
J. Clin. Endocrinol. = Journal of Clinical Endocrinology (later, J. Clin. Endocrinol. Metab.)
J. Clin. Endocrinol. Metab. = Journal of Clinical Endocrinology and Metabolism
J. Clin. Invest. = Journal of Clinical Investigation
J. Embryol. Exp. Morphol. = Journal of Embryology and Experimental Morphology
J. Exp. Med. = Journal of Experimental Medicine
J. Exp. Zool. = Journal of Experimental Zoology
J. Gen. Physiol. = Journal of General Physiology
J. Gerontol. = Journal of Gerontology
Physiol. Rev. = Physiological Reviews
Proc. ——— Internat. Conf. Planned Parenthood = Proceedings of the —— International Conference on Planned Parenthood.
Proc. ——— Internat. Congr. Hormonal Steroids = Proceedings of the —— International Congress on Hormonal Steroids

Proc. ——— Internat. Physiol. Congr. = Proceedings of the ——— International Physiological Congress
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. 6th Pan-Am. Congr. Endocrinol. = Proceedings of the 6th Pan-American Congress of Endocrinology
Proc. Roy. Soc. London = Proceedings of the Royal Society of London (Ser. A = Mathematical Sciences; Ser. B = Biological Sciences)
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
Psychosomat. Med. = Psychosomatic Medicine
Recent Progr. Hormone Res. = Recent Progress in Hormone Research

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A. N. Richards

ALFRED NEWTON RICHARDS

March 22, 1876–March 24, 1966

BY CARL F. SCHMIDT

ALFRED NEWTON RICHARDS was born in Stamford, New York, on March 22, 1876, the youngest of three sons of Rev. Leonard E. and Mary Elizabeth (Burbank) Richards. His father was in the fourth generation of a line of Ohio farmers, the first of whom (Godfrey Richards) came to America from the Rhenish Palatinate about 1740. Leonard E. Richards apparently was the first of the line to leave the farm for the university. He worked his way through Ohio Wesleyan University and Union Theological Seminary and was pastor of the First Presbyterian Church in Stamford from 1864 until his death in 1903.

Mary Elizabeth Burbank was the daughter of Rev. Caleb Burbank and Delpha Harris (Burbank), both descendants of emigrants from England to New England prior to 1640. Her father was a graduate of Dartmouth College and Andover Theological Seminary and her maternal grandfather (Luther Harris) was a member of the faculty of Providence College, which subsequently became Brown University. She herself gradduated from Granville Female Seminary (subsequently incorporated into Dennison College) and taught school in Norwalk, Ohio, prior to her marriage. During this period she lived in the home of Rev. Alfred Newton, who is still remembered as one of the most beloved and influential men ever to live in Norwalk. His daughter Martha was the future Mrs. Leonard Richards' best friend, and memories of those days evidently impelled the latter to choose the name Alfred Newton for her third son.

The father of the three Richards boys was reserved and strict, the religious training severe, the life austere. The family's income during most of the Stamford period was less than $1000 a year, but they kept a cow for milk and butter and they depended on their own large garden for vegetables. In his notes on this period, Dr. Richards wrote: "We were poor, but like Eisenhower's folks were not aware of it. On his small salary my father and mother managed to save enough to help three sons through college and when father died, he left enough to keep my mother in her home in Stamford entirely independent of help from her sons."

Except for brief periods, such as the arrival of a new baby, there was no domestic help. Mrs. Richards did all the housework (cooking, baking, washing, ironing, cleaning), taught in Sunday school, played the organ and conducted the choir in the church, and was deeply involved in community affairs, especially in those which concerned the school and church. She loved music and learning and did much more than her husband to advance the education of their three sons.

Alfred Newton Richards learned to read at home before he was five years old, and at the age of six he proceeded to the Stamford Seminary and Union Free School, an institution established largely as the result of his father's effort and influence. He went through all the grades and graduated in 1892 as valedictorian of his class, a distinction which (according to his notes), "considering the competition, doesn't mean much."

His brother James at that time was a student at Yale, and although Alfred Newton at age sixteen was not deemed ready for college, he went to New Haven in June 1892 to take preliminary entrance examinations. In two and a half days he took eighteen examinations and succeeded in passing six. A year later he returned to take the twelve examinations he missed previously and a few others he had not been permitted to take in 1892. He did well enough to be assigned to the "first division" and in the autumn of 1893 he was admitted to the class of 1897 of Yale College.

According to his notes, he enjoyed most of the subjects and did well enough to escape any conditions, but not quite well enough to be chosen for Phi Beta Kappa, which was an intense disappointment to his mother. Like all his contemporaries in Yale College, he was not permitted to elect science courses until the third year, when he began chemistry (inorganic and organic) and physics. He was fascinated by the laboratory exercises in chemistry and did some special work that enabled him to graduate in June 1897 with honors in chemistry, which pleased his parents. He did poorly in physics, partly because he discontinued mathematics after the second year, to his subsequent regret.

The great experience of his college years at Yale—and indeed the turning point of his career—was a course in physiological chemistry given by R. H. Chittenden in the Sheffield Scientific School, which then was completely separate from the college. This course was intended primarily for seniors in the college who planned to study medicine, and at the time young Richards regarded himself as one of these. Shortly before his graduation, however, his father let him know that he could expect no further financial assistance and was now on his own.

Chittenden at that time was the leading authority in America on the new science of physiological chemistry. Trained in the rigid requirements of contemporary German laboratories, he was a stickler for accuracy and thoroughness and his laboratory was so operated as to create an ambition for perfection. He also took a personal interest in his students, and so it was natural for him one day in the spring of 1897 to ask young

Richards what he intended to do after graduation. When Richards replied that his original plan to study medicine had to be abandoned for lack of funds, Chittenden offered him a fellowship for a year of graduate study in physiological chemistry in the Sheffield Scientific School. The fellowship covered only tuition, but Richards worked in a summer hotel to raise a little money, got a job waiting on tables in a students' boardinghouse in New Haven to provide room and board, and with a little extra help from his father managed to finance the year 1897-1898 in Chittenden's laboratory.

During this year young Richards was assigned a series of tasks having to do with quantitative chemical methods and he learned that, if he took enough pains, he could do things on his own and do them well. The great experience, however, was a study of the starch-digesting power of human saliva (his own) under different circumstances. This work brought him into close association with Chittenden, whose impressions, judging from the outcome, must have been favorable. It also led to a joint paper in Volume 1 of the new *American Journal of Physiology*, a fact that places Richards among the pioneers of American physiology.

During the spring of 1898 Chittenden was invited to undertake the reorganization of the Department of Medical Chemistry in the College of Physicians and Surgeons of Columbia University in New York. He accepted, subject to the understanding that he would spend only one day a week in New York, when he would lecture to the medical students and consult with the permanent staff. The members of this staff were to be selected from his laboratory at Yale, and Richards was invited to be one of them, at the princely salary of $800, which was far more than he had been accustomed to spend.

So in September of 1898 he began a ten-year association with the old College of Physicians and Surgeons on West 59th Street in New York. During the first six of these years he was an assistant and tutor in physiological chemistry, while for the last four he was an instructor in pharmacology. At first his only connection with the teaching of physiological chemistry was to attend to the preparations for the laboratory exercises of the medical students, to circulate among them in the laboratory to answer questions, and to help whenever occasion arose. He was also permitted to enroll in the Graduate School of Columbia Univeristy as a candidate for the Ph.D. degree, with a major in physiological chemistry and minors in physiology and bacteriology. When he got his Ph.D. in 1901, he was the first ever to receive this degree from the Department of Physiological Chemistry at Columbia. His thesis was on "The Composition of Yellow Elastic Connective Tissue," a topic selected by W. J. Gies, his supervisor at Columbia. It represented a great deal of work and provided a valuable experience in quantitative chemical methodology but, after publication in Volume 7 of the *American Journal of Physiology*, it had no further influence on his career.

A course in bacteriology under Philip Hanson Hiss was a different story. Hiss was interested at the time in the development of differential culture media, in which certain microorganisms would grow while others would not. He called on Richards to provide him with glycogen, which the young biochemist isolated from scallops. Then he asked for a carbohydrate that would yield only levulose on hydrolysis and Richards, having learned from the literature that inulin should meet this requirement, provided him in 1901 with pure inulin separated from Dahlia bulbs. Some thirty years later, when Richards was looking for a diffusible polysaccharide which would not be digested in the blood or in the lumen of the frog's kidney, he thought of inulin, tried it, and (with Westfall and Bott) began to study the renal clearance of inulin in dogs at least a year before the same procedure was attempted by others.

Richards' work with Hiss attracted the attention of

T. Mitchell Prudden and Christian A. Herter, prominent members of the Columbia faculty who in 1901 were about to become trustees of the nascent Rockefeller Institute for Medical Research. The result was a Rockefeller scholarship which enabled Richards to spend the next year working with Herter in the Laboratory of Physiological Chemistry at Columbia on a problem related to the glycosuric effects of adrenaline locally applied to the pancreas of the dog.

Thus in the autumn of 1901 young Richards' career took another turn. The new work was much more interesting than what he had been doing. He got along well with Christian Herter, a man of independent means in whose home he made the acquaintance of such men as Emmett Holt, Park, Jacques Loeb, Ostwald, Ehrlich, Meyer, and numerous musicians, artists, and writers who were Herter's friends. In 1902 he first met George B. Wallace, who had just come to New York from Cushny's Department of Pharmacology at Michigan to introduce pharmacology as an experimental science in the Bellevue Medical School. Richards and Wallace took to one another from the start and soon were sharing a room in a boardinghouse on 46th Street. Until his death in 1949, Wallace was Richards' closest friend.

In 1903 Herter accepted an invitation to become Professor of Pharmacology and Therapeutics at Columbia. He persuaded Richards to accept an instructorship in the new department and assigned him the job of organizing a laboratory course in pharmacology corresponding with the one recently introduced by Wallace at Bellevue. Neither Herter nor Richards pretended to know anything about pharmacology, and so they were given a year's leave of absence in which to prepare for their new assignments. Wallace joined Richards in a trip to Strassburg in June 1903, where they hoped to learn how to organize a student laboratory course in pharmacology from Oswald Schmeideberg, whose laboratory then was the mecca for those seeking preparation for careers in this new science. They were disappointed to find only uninspired lectures and rather pedestrian research "Arbeiten," but Richards relieved the tedium of Schmiedeberg's laboratory by spending the afternoons in the chemical laboratory of Franz Hofmeister, whom he found as interesting and stimulating as Schmiedeberg was the reverse.

In the early autumn of 1903 Richards returned to New York without having learned more pharmacology than he could have gleaned from Schmiedeberg's book, and with no progress in the job of preparing a laboratory course in pharmacology for the following year. Herter was spending the entire year learning pharmacology from Ehrlich in Frankfurt and Meyer in Marburg and was not available for advice or assistance. Richards therefore turned to his friend George Wallace and got his permission to assist in the student laboratory exercises in pharmacology at Bellevue. Thus he learned to carry out a number of relatively simple student experiments. This experience, together with studying textbooks and reading some of the literature, constituted his formal training for a career in pharmacology.

Later in the academic year President Nicholas Murray Butler of Columbia wrote Richards that an increase in admission requirements for the Law School would lessen the University's income to an extent that would make it impossible to go through with the plans for a new Department of Pharmacology. By this time Richards had already resigned from the Department of Physiological Chemistry and was faced with the need to justify a promised salary in pharmacology. On Wallace's advice, he sought and got the permission of John G. Curtis, head of the Department of Physiology at Columbia, to use the newly equipped student Laboratory of Physiology for an elective course in pharmacology for twenty-four students.

President Butler and Herter approved the plan, which Richards proceeded to carry out.

An interlude of this period began with an encounter in 1903 between Richards and John Howland, who was beginning a distinguished career in pediatrics as assistant to L. Emmett Holt. Howland was trying to improve the treatment of a child suffering from cyclical vomiting by searching the urine for substances other than acetone and acetoacetic acid, which had recently been reported to be present. Howland's obvious inexperience in chemistry led Richards to offer his assistance and between them they soon demonstrated the presence in the urine of large amounts of indican (indoxyl sulfuric acid) in addition to ketone bodies. The latter were then regarded as evidence of decreased oxidations in the liver, while the former was known to be an oxidation product of indole, a product of intestinal putrefaction. These considerations led Richards to propose that cyclic vomiting in children might be related to failure of the liver to oxidize indole, a substance that can cause muscular twitchings and convulsions in animals, and to design animal experiments to test the proposition.

The idea was to investigate the influence of interference with oxidations (by means of cyanide) on the toxicity of indole. Preliminary experiments on frogs and guinea pigs indicated that the effects of indole can be prolonged by simultaneous injections of nonlethal doses of cyanide. When similar experiments were made in dogs, an unexpected chronic posioning ensued, characterized by spasticity of muscles, vomiting, and blindness. This lasted several days and was invariably followed by death.

These findings were judged important enough to warrant a report by Howland at the meeting of the American Pediatric Society in June 1904. Unfortunately, Howland's first child was born the night before the meeting and the Society's regulations precluded presentation of the paper by Richards, who was not a member. He was, however, permitted to join in the discussion after the report was read by a member of the Society. Thus he had his first encounter with David Edsall, who was greatly interested in the findings and apparently also impressed by the young chemist-pharmacologist who was instrumental in obtaining them.

Richards and Howland then extended their experiments to include other means of inhibiting oxidations in the liver, and thus they came to a series of observations with chloroform, whose ability to produce acute yellow atrophy of the liver had recently been reported. They spent considerable time on a study of the late effects of chloroform on the liver, including urinalyses and tissue studies. By this time (1908) Richards had accepted an appointment in Chicago and he and Howland decided to compile a report on their indole work. Before doing this Richards characteristically insisted that they repeat the earlier experiments, to be certain that the results could be duplicated by anybody who cared to make the effort. To their horror, they were unable to reproduce the delayed fatal poisoning by cyanide and indole. After many unsuccessful attempts to find the reason for the discrepancy, they finally decided not to attempt a definitive report and the results of this effort were never published.

Nevertheless, the four years of joint effort with Howland were far from wasted. The work on delayed chloroform poisoning led to a publication that attracted considerable attention. Richards' concurrent elective course in pharmacology for medical students entailed a continuing challenge to increase his skill in methods that might be useful in investigations of drug actions, and among these methods perfusion techniques were prominent. The Langendorff procedure for maintaining the functions of the perfused mammalian heart had recently been used to study the actions of chloroform on the mechanical and metabolic functions of the heart, and the possibility of using a corresponding procedure to elucidate the effects of chloroform and other drugs on the liver gave rise to a plan for the perfusion pump which was eventually to determine the course of his career. His connection with the study on chronic indole poisoning as a possible cause of cyclic vomiting in children attracted the attention of David Edsall, who as Professor of Therapeutics at the University of Pennsylvania was instrumental in getting Richards to come to that institution six years later. Richards' association with Howland gave him his first direct contact with clinical problems and was the start of a plan for a collaborative clinical and laboratory activity which eventually became a program in clinical pharmacology, the first of its kind in the country if not in the world. The paper on chloroform poisoning led George Whipple to use this procedure in experiments which began the researches that eventually brought him the Nobel Prize. John Howland got an apprenticeship in quantitative chemical methodology which stood him in good stead in the studies of metabolic diseases in children which subsequently made him famous.

Meanwhile, as Instructor in Pharmacology, Richards was continuing the elective course in pharmacology in the Laboratory of Physiology. This course must have been successful because, in the spring of 1907, the dean of the Columbia Medical School asked Richards to make it a part of the regular course for second-year medical students. There being almost no money available for such purposes, Richards agreed to equip a laboratory for twenty-four students for $500. After a visit to inspect Cushny's laboratory at Ann Arbor, he designed and arranged for the building of six animal tables and some cabinets in his home town, where carpenter work was cheap. These articles were shipped knocked down to New York, where they were assembled by Richards and a retired locomotive engineer, who was hired as diener. Animal holders were purchased from the Department of Physiology, kymographs from the Harvard Apparatus Company, and Richards himself made mercury manometers and sphygmomanometers. Through such improvisations he got the laboratory going on less than $500.

At the end of the first year of this arrangement (i.e., spring, 1908) Richards was offered an assistant professorship of physiological chemistry in the University of Missouri and the professorship of pharmacology at Northwestern University. At first he refused both offers because he was so interested in what he was then doing that he did not want to leave New York. However, he had recently become engaged to Lillian Woody, and when these two tried to foretell when they might be married on what he was likely to earn in New York, they decided to accept the offer from Northwestern. So in August 1908, he and Wallace broke up their partnership in the flat they had shared for six years and he moved to Chicago.

During the ten years he spent in New York (1898-1908) Richards saw and participated in an unparalleled series of events in medical education and research which marked the emergence of the modern pattern, and of himself as one of its most promising young advocates. The decade began with the appearance of his first scientific publication in Volume 1 of the *American Journal of Physiology,* the first of several new periodicals to appear in rapid succession as manifestations of the growing interest in research in the medical sciences in the United States. This paper had to do with the digestion of starch by saliva and its appearance in the new physiological journal places it in the period preceding the separation of the chemical approach to physiology into a distinct scientific discipline, biochemistry. Actually he was soon to play an active part in this separation, and his next contributions to the physiological literature would be along lines other than chemical.

Richards was one of the first scholars of the Rockefeller Institute for Medical Research, which began its distinguished career in 1901. As a Rockefeller Scholar he was working with

Christian Herter in 1904 when the latter was invited by John J. Abel to join in founding a new *Journal of Biological Chemistry*. Abel, who at that time was in charge of both pharmacology and physiological chemistry at Johns Hopkins, had recently been instrumental in establishing the *Journal of Experimental Medicine*. Abel and Herter became joint editors of the new *Journal of Biological Chemistry* and Richards was invited to serve as assistant editor, which involved responsibility for proofreading, preparation of manuscripts for the printers, and business transactions with the publishers. The first number appeared late in 1905 and the journal, now in its 246th volume, has become one of the most important scientific publications in the world. It is noteworthy that, on his retirement as managing editor in 1914, Richards received letters of appreciation from contributors who gratefully recalled instances in which the meticulous, conscientious young managing editor had pointed out errors or misstatements in texts or tables before publication.

The spirit of the times is evident in the ability of John Abel, head of the Department of Pharmacology in the recently organized Johns Hopkins Medical School, not only to direct the Department of Physiological Chemistry as well as his own, but also to bring about the establishment of two of the most influential American scientific journals. As a matter of fact, in 1909 the same man proceeded to establish a third—the *Journal of Pharmacology and Experimental Therapeutics*—but by this time Richards was in Chicago and had nothing to do with the new undertaking. These events were concurrent with the establishment of the American Society of Biological Chemists (1906) and the American Society for Pharmacology and Experimental Therapeutics (1908), as offshoots of the American Physiological Society, which had been in existence since 1887. The next development was the coalescence of these three societies into the Federation of American Societies for Experimental Biology, which took place in Philadelphia in 1913, three years after Richards had moved to that city.

Meanwhile the dynamic new spirit which underlay these events was transforming the medical scene in New York. Its impact was first apparent in the Bellevue School, and the development of major importance to Richards was the coming of George Wallace from Cushny's Department of Pharmacology at Michigan in 1902 to set up the first teaching department in the science of pharmacology in the New York area. The Rockefeller Institute then was under construction and in 1904 Simon Flexner arrived as its director, bringing with him Opie and Noguchi. Meltzer, Levene, and others soon were drawn into the new enterprise. Two new societies which began as strictly local organizations but have grown to have much more than local significance—the Society for Experimental Biology and Medicine, and the Harvey Society—were founded in New York in 1904 and 1905 respectively. The list of research men involved in these developments includes Chittenden, Gies, Prudden, Herter, Lusk, Dunham, Hiss, Wallace, Shaffer, Flexner, Meltzer, Jacques Loeb, Opie, and Noguchi. In his notes, Richards has this to say: "I had the privilege of intimate association—on a somewhat junior basis—with all these people, and became saturated with their ideals. What I accomplished by way of lastingly useful scientific research was negligible but what I got by way of spiritual stimulation was beyond price."

Richards left for Chicago on August 1, 1908, to prepare the laboratory for the opening of classes in September. Pharmacology previously had been combined with physiology at Northwestern and Richards' appointment was intended to give greater recognition to pharmacology, which then was being projected into new prominence by the work of Dale on the actions of natural and synthetic sympathomimetic amines and by that of Ehrlich on the synthesis of arsenical chemotherapeutic agents. Richards was expected to conduct laboratory exercises for classes of about 150 medical students in materia medica and pharmacy, as well as in experimental pharmacology. There was one large room, half of which was equipped with chemical benches and lockers. The other half was vacant except for the professor's desk, which was railed off in one corner. The chemical desks were well made but badly scarred and the energetic new professor, accustomed to smooth chemical black-tops, immediately set to work (with one ignorant and inadequate diener) to remove the unsightly old finish and replace it with a shiny new one. This was accomplished in time for the first semester and the new appearance of the laboratory elicited favorable comments from faculty, students, and visitors, one of whom was Richards' former teacher Chittenden.

Richards had had no instruction or experience in materia medica and pharmacy, but he went about the new task with characteristic energy and dedication and he quickly won the cooperation, respect, and eventually the admiration of the students. For the course in experimental pharmacology in the second semester he prepared much the same laboratory exercises as those he had learned from Wallace. He had the part-time assistance of one third-year medical student and one diener. The class had to be handled in six sections, which meant a student laboratory exercise every day of the week except Sunday. He gave two lectures and conducted two conferences a week. He also managed to keep up with the managing editorship of the *Journal of Biological Chemistry*. On December 26, 1908, he was married to Lillian Woody and set up housekeeping in an apartment on Michigan Avenue.

Research was out of the question except during the summer, when he attempted to learn if acquired tolerance to alcohol would make the heart of an animal resistant to the depressant effects of the drug. The results were inconclusive, but the experience in overcoming the vagaries of the isolated mammalian heart preparation helped to make this experiment one of the memorable events of medical students in his laboratory.

The second year in Chicago was much like the first. During its course he was invited by David Edsall, Professor of Therapeutics at the University of Pennsylvania, to come to that institution to reorganize the Department of Pharmacology. Pennsylvania—the oldest medical school in North America—was at last becoming aware of the ferment that Richards had already experienced in New York and a group of young faculty members was engaged in an attempt to introduce some of the new research attitudes, which had not yet had much impact in Philadelphia. Their immediate program involved the installation of Edsall as Professor of Medicine, his friend Alonzo Engelbert Taylor as Professor of Physiological Chemistry, Howard Ricketts as Professor of Pathology, Richard M. Pearce in a newly endowed chair of research medicine, and Richards as Professor of Pharmacology.

Except for the appointment of Ricketts (who died in the summer of 1910) this program was carried out, and Richards came to Pennsylvania in the autumn of 1910 as one of a small group of devotees of the then new research approach to the teaching and practice of medicine. They were brought there through the efforts of a small but influential minority of the faculty and they were confronted with the hostility of a majority who were satisfied with things as they were. Edsall, the senior member of the group and the one from whom most was expected, stayed one year in this new post and then resigned to go on to a distinguished career at Harvard. Taylor soon left to join his fellow Californian, Herbert Hoover, in relief activities during World War I and transferred his vigor and talents to nutritional economics at the national and international level. Pearce secured support from the Rockefeller Foundation and practically withdrew from university activities. Only Richards, the youngest and most inconspicuous of the group, and the only one without medical training in an institution with a long tradition of clinical excellence, stayed on to win local, national, and international fame for himself, his laboratory, and his adopted university.

The first year in Philadelphia was very difficult. Pharmacology had been allotted considerable space in a new Medical Laboratories Building opened in 1906, but the student courses that had been given followed the traditional materia medica approach and had established a reputation for being uninteresting and unimportant. Richards set to work to make over the laboratory so as to permit mammalian pharmacological experiments by the students, which had not been attempted previously at Pennsylvania. He also insisted that his course should be given to the second-year students rather than the third, as had been the custom. The change meant giving the course twice during 1910-1911, and this was done. He had two assistants to help in the laboratory and run the student conferences, but neither had had any previous experience with the type of course Richards wished to give and he had to begin by doing everything himself.

To complicate matters further, Christian Herter died in December 1910 and the editorship of the *Journal of Biological Chemistry* had to be reallocated. The business and editorial offices were immediately transferred to Richards' laboratory and he became in effect managing editor, with a secretary and an editorial assistant. This arrangement continued until 1914, by which time the enterprise had come to occupy so much of his time that he had to choose between it and serious research. He gave up the *Journal.* The new editor was Donald Van Slyke and the offices were moved to the Rockefeller Institute in New York.

The year 1910-1911 was the nadir of his career at Pennsylvania. During this time his new-type course in pharmacology began to arouse interest, respect, and eventually admiration in the students, who were having their first experience with the modesty, self-criticism, and objectivity of a true scientist. In 1911 he was joined by Oscar H. Plant, who had been teaching pharmacology at the University of Texas. Plant soon proved able and willing to take over the tasks of preparing and conducting the student laboratory exercises, but Richards continued to give all the lectures and to participate in the student experiments. This, with his editorial duties, left no time for research.

As a matter of fact the research program that eventually brought him distinction developed naturally from his unremitting efforts to improve his teaching, perfection of which dominated his existence then and for years to come. The first of a series of pertinent events came in June 1913, when he was approached by a member of the graduating class at Pennsylvania who had found Richards' course in pharmacology so interesting that he wanted more experience in this field before he went to Johns Hopkins to spend a year with Howell in physiology. His name was Cecil K. Drinker and his academic record in the medical school at Pennsylvania marked him as one of the most brilliant students ever enrolled there. After considering several possible research projects, Richards and Drinker decided to undertake construction of the perfusion pump whose general design had been developing in Richards' mind ever since his work with Howland on chloroform poisoning of the liver. It turned out to be a fortunate choice because Drinker had considerable skill in working with metals. The result was a device by which mammalian organs could be perfused with blood in patterns so close to the normal as to permit useful physiological studies in tissues isolated from the body. The functional adequacy of the pump was demonstrated before Drinker left for Baltimore in the autumn of 1913. Drinker went on to achieve distinction in his own right, but he also happened to be the first of a considerable number of medical graduates who came under Richards' influence at early stages in their careers and who contributed to the reputation of his laboratory either directly or through protégés recommended to him. Drinker's greatest contribution to Richards' subsequent rise to prominence actually was his recommendation to two recent graduates of the Harvard Medical School—Joseph T. Wearn and Arthur M. Walker—that they spend a year studying pharmacology at Pennsylvania before settling down to careers in academic medicine. These two men subsequently played preponderant roles in the research projects from which Richards' fame was derived.

Drinker left for Baltimore before the new perfusion pump could be used for more than validation studies. After discussing the matter with Plant, Richards decided in 1913 to utilize the pump to settle once and for all the question of how caffeine causes diuresis. At that time the physiology of urine formation was locked in controversy between the protagonists of the filtration-reabsorption theory of Ludwig and the secretion theory of Heidenhain. Neither side had been able to devise a crucial experiment and Richards therefore was compelled year after year to give the medical students the two alternatives, while refraining from expressing a conviction of his own. The new pump would make it possible to maintain a constant flow of blood through the kidney irrespective of changes in the resistance of the renal blood vessels. Increased urine from caffeine under these circumstances would be evidence favoring stimulation of secretory activity while absence of effect would argue for the filtration-reabsorption concept.

The experimental difficulties turned out to be unexpectedly great. This was long before the discovery of heparin and the only nontoxic anticoagulant available for viviperfusion experiments was hirudin (leech head extract), which was very difficult to obtain after the outbreak of World War I in July 1914. Finally Richards and Plant completed a small series of experiments in which caffeine was found to increase urine formation by the mammalian kidney perfused with a constant volume of blood and diuresis was associated with fall rather than rise in renal arterial pressure. These findings were favorable to the Heidenhain secretion theory and irreconcilable with the Ludwig filtration-reabsorption concept as then formulated. Richards was unwilling to let the matter rest there, but further work had to be postponed until World War I had ended.

Shortly after America became involved in World War I, Richards was invited to join Henry Dale in a study, under the auspices of the British Medical Research Committee, on the cause of wound shock. One result was a definitive investigation of the actions of histamine, which led to a publication that is one of the classics of medical literature. One of the novel conclusions was that the capillaries are not entirely passive, but have considerable capacity for intrinsic control. This idea had immense influence on Richards' subsequent work on kidney function.

In July 1918 Richards was commissioned a major in the U.S. Sanitary Corps and given the assignment of setting up a field laboratory for the study of problems of chemical warfare at Chaumont, France. He went to France early in September and was at Chaumont when the armistice ended the war in November 1918. Shortly thereafter he returned to Philadelphia to resume his professional duties, from which he had been granted leave of absence by the university authorities, and was given an honorable discharge from the U.S. Army in Washington in December 1918.

He plunged immediately into the teaching program, which was arousing greater and greater admiration as time went on. He and Plant also resumed the kidney perfusion experiments they had begun five years previously.

This time the plan was to maintain constant the quality and quantity of blood flowing through a rabbit kidney while raising the renal arterial pressure by several different procedures. These procedures included partial obstruction of the renal vein, stimulation of the splanchnic nerve, and introduction of minimal vasoconstrictor doses of adrenaline. The common result was diuresis, a finding that was regarded as cogent support for the Ludwig filtration-reabsorption concept of urine formation.

These experiments were demonstrated to a number of visiting scientists, one of whom suggested that a concomitant record of kidney volume by an oncometer might provide useful information. When this was done, it was found that while adrenaline in more than minimal dosage caused rise in renal arterial pressure, fall in kidney volume, and anuria, the smallest effective amounts cause rise in arterial pressure, together with increase in kidney volume and diuresis. In corresponding perfusions of the leg, any dose of adrenaline that raised the perfusion pressure caused a pure decrease in limb volume.

These findings suggested to Richards that the paradoxical behavior of the renal circulation following minimal vasoconstrictor doses of adrenaline might be related to the presence of glomerular capillaries intercalated between afferent and efferent arterioles. More specifically, if the efferent glomerular arteriole were constricted a little more than the afferent by minimum effective concentrations of adrenaline, the swelling of the kidney could be attributed to increased glomerular volume, the diuresis to increased glomerular filtration pressure. This general idea has become a familiar and important part of modern nephrology, but at that time it was a totally new concept which Richards was unwilling to consider further without direct experimental proof or disproof. It was against this background that the experiments that made him and his laboratory famous came to pass.

In 1919 Richards' budget was increased enough to enable him to take on a new instructor, whose accession increased the number of his scientific staff by 50 percent, that is, from two to three.

It was my good fortune to be chosen to fill this new vacancy. As a second-year medical student at Pennsylvania, I had taken the course in pharmacology which he and Plant gave and my recollections caused me to want further experience with such work. I remembered that in pharmacology the equipment always worked and the experiments almost always succeeded; when they did not, the laboratory reports came back with painstaking and informative explanations and discussions. I did not realize, until I joined the department, the enormous effort that underlay this smooth performance. Richards set an exhausting pace of attention to detail and rehearsal to the point of perfection. He used to say that the major function of a teaching department was to teach, and that the First Law of Thermodynamics applied as much to teaching as to research, which I took to mean that one got out of teaching precisely what one put into it. I was continuously challenged to try to live up to the standards of excellence he set for himself.

He regarded research in his department as a privilege rather than a right or duty, and he encouraged us recent medical graduates to develop our own projects while trying to blend them into the teaching program as far as possible. I later found that this procedure served the dual purpose of giving the students the benefit of the most recent local advances and of maintaining a peak level of interest of the teaching staff in their assignments. He sought to gain, hold, and deserve a reputation for keeping his teaching program up to date, and in this he succeeded. At the time, this was a novel approach, and the success that was soon to crown his career was, in my opinion, due more to his emphasis on excellence in teaching than to any other single factor.

When I joined Richards' department in the autumn of 1919, he and Plant were finishing the pump perfusion experiments on the kidney that led to the hypothesis that adrenaline, in minimum effective dosage, constricts the efferent glomerular arterioles relatively more than the afferent. Richards did not attempt at the time to elaborate the intellectually stimulating implications of this hypothesis as others have done since, but set himself the task of finding experimental evidence that would prove or disprove it. We all were busily studying Cushny's recent monograph, *The Secretion of the Urine,* and while so doing one of us came upon an inconspicuous account of recent experiments by Ghiron, who claimed to have watched the kidney of a mouse under the microscope while the glomeruli emerged as small dark globules after intravenous injections of dyes. Urged by Richards to try this for ourselves, we were unable to confirm Ghiron's observations, whereupon Richards suggested that the flat kidney of the frog might be more favorable than the globular organ of the mouse. This turned out to be the case and so during 1919-1920 I spent most of my free time watching the circulation on the surface of the frog's kidney under the low-power microscope.

The original purpose was to see if the glomeruli actually increased in volume under the influence of minute doses of adrenaline, but we forgot all about this in our entrancement over hitherto unknown events in the renal circulation, including intermittence of blood flow in different glomeruli and changes in the pattern of flow in individual loops of single glomeruli. The equipment was improvised, my experience nearly nonexistent, and I wound up with a good deal of eyestrain from uneven reflection of intense light from the surface of the frog's kidney. Therefore I was glad to withdraw from this project the next year when Joseph Wearn came from

Harvard to spend two years with Richards. By this time the departmental budget had been increased enough to provide a new instructor to help me in another project.

Wearn quickly improved the technique for microscopic observation of the circulation on the surface of the frog's kidney, but his greatest contribution was his suggestion that fluid might be directly withdrawn from the glomerular space for chemical analysis. At a meeting in Philadelphia in December 1920, he and Richards saw a demonstration by Robert Chambers of a new micromanipulating technique, and Richards, thinking of his urgent desire to demonstrate swelling of the glomerulus by the action of adrenaline on the efferent arteriole, proposed that the Chambers technique be used to inject a minute dose of adrenaline through the glomerular capsule while observing the area under the microscope. Wearn thereupon made the countersuggestion that if this could be done without damaging glomerular structures, the same technique might be used to withdraw fluid directly from the glomerular space for chemical analysis.

The result was one of those simple, direct, unambiguous experiments that researchers dream of but seldom attain. Glomerular fluid was readily collected in volume sufficient for qualitative tests for chloride (by silver nitrate) and sugar (by Benedict's solution) and both tests were positive. Bladder urine, simultaneously collected, gave negative reactions to both. Obviously these normal constituents of the blood were present in the glomerular fluid but were reabsorbed between the glomerulus and the bladder. The result was the first direct and unambiguous evidence of tubular reabsorption, which suggests (though it does not prove) glomerular filtration.

These experiments were reported and demonstrated at the annual meeting of the American Physiological Society in New Haven in December 1921, where they shared the spotlight with the first public announcement of the successful isolation of insulin. Richards' interpretation of his findings as evidence in support of the filtration-reabsorption concept of kidney function in general was challenged on the ground that evidence from the primitive kidney of the frog is not necessarily applicable to the behavior of the more complex mammalian kidney. To this criticism he replied that he and his colleagues had tried to make corresponding observations on the kidneys of mammals but had been unsuccessful. Since the kidney of the frog clearly subserves functions analogous to, if not identical with, those subserved by a mammalian kidney, they had decided that it was better to make the observations on the frog than not to make them at all. This rebuttal won an ovation the like of which I have never seen before or since. I do not know how extemporaneous his discussion was on that occasion but if it was prepared it was done with characteristic foresight, intelligence, and effectiveness.

The final answer to this particular objection was not obtained until nearly twenty years later, when Richards' senior assistant Arthur Walker, assisted by Jean Oliver and Phyllis Bott, succeeded in the incredibly difficult task of collecting and analyzing fluid from the glomeruli and various tubular levels of the kidneys of rats, guinea pigs, and opossums. The findings confirmed those on amphibia in all essential respects.

Before this occurred, however, a number of other important events took place. In 1921 the University increased his budget to enable him to begin a long-planned program in clinical pharmacology. Thus he was able to add to his staff recent medical graduates who, after a few years of full-time participation in the teaching and research activities of the Department of Pharmacology, went on to junior positions in the clinical services of the University Hospital. They continued to participate in the teaching and research programs in pharmacology and they organized and conducted a course in clinical pharmacology for the third-year students. This undertaking was highly successful, largely because it began by attracting such men as Isaac Starr, Joseph Hayman, Hugh Montgomery, Kendall Elsom, Osler Abbott, John Barnwell, and Arthur Walker. Starr, Hayman, Barnwell, and Walker joined Wearn and Richards in the kidney research project, which now went from the qualitative to the quantitative stage.

This step, which eventually involved quantitative determinations of the amounts of different urinary constituents in volumes of fluid of the order of one cubic millimeter or less, was made possible by Richards' observation of the remarkable accuracy with which his eye could distinguish small differences in the colors of fluids contained in glass tubes 0.5 mm. in diameter. By ingenious and painstaking modifications of existing chemical methods, he and his colleagues succeeded in making quantitative measurements of the content of eleven separate urinary components (glucose, chloride, urea, uric acid, creatinine, ammonia, alkali, phosphates, sulphates, total molecular concentration, and protein) in fluid samples collected from the glomerular spaces and from various levels of the corresponding renal tubules. The studies were first made on frogs, then on snakes and salamanders (necturi), and eventually on mammals. Corresponding methods also were developed for various foreign substances (dyestuffs, sucrose, xylose, inulin).

The major conclusion from all these efforts was that the renal excretion of substances normally found in the body involves separation from the glomerular capillaries of large amounts of an ultrafiltrate of blood plasma. Everything normally found in the urine was shown to be present in the glomerular fluid, and in concentrations essentially the same as in the blood plasma. In their course down the tubule the different components of the glomerular filtrate are reabsorbed into the blood stream at different sites and to different degrees ranging from complete (glucose) through nearly complete (water, alkali) to little or not at all (nitrogenous wastes and polysaccharides such as sucrose and inulin). Foreign substances (such as phenol red) can be secreted by the walls of the tubule into the fluid in the lumen, but this was not seen with any normal constituents of the urine and Richards resisted the claims of others that active secretion plays an important part in normal kidney function. He conceded its occurrence in fish with aglomerular kidneys or with foreign substances, and he himself provided a simple, direct, and conclusive demonstration of tubular secretion of a synthetic dye by immersing the freshly excised kidneys of frogs in a very dilute solution of phenol red. In about thirty minutes red streaks were seen in the kidneys and he proved these to be due to dye concentrated in the tubular lumina by injecting fluid under pressure through the ureter. This caused the streaks to be transformed into small spheres, representing the glomeruli with which the tubules were connected. Low concentrations of cyanide prevented the phenomenon, indicating an energy-coupled mechanism. Small amounts of mercuric chloride (a notorious kidney poison) acted like cyanide.

This particular observation gave rise to a study which proved that the decreased renal excretion of water and solutes, the most dangerous effect of mercury in the body, is due to interference by mercury with the selective functions of the renal tubular epithelium. As a result, the entire glomerular filtrate diffuses back into the peritubular circulation without regard to the body's requirements and death ensues from retention of water and waste products that normally would have been held back in the tubules for excretion in the urine. These findings, which were accepted as illuminating the events of "lower nephron nephrosis" in general, increased further the already considerable influence of the contributions of Richards' laboratory on advances in clinical medicine.

By 1924 Richards had become one of the most influential members of the Pennsylvania medical faculty. The process was aided by a close personal friendship, based on mutual respect and admiration, between Richards and William Pepper, who, soon after he became dean of the Medical School in 1912, learned to turn to the self-effacing young Professor of Pharmacology for advice on administrative problems and policies. It was a period of rapid acceptance and growing support of medical research at Pennsylvania, which was being led out of its former lethargy by the gentle, wise leadership of the new dean. Major steps were the appointments in 1921 of Henry C. Bazett (an associate of J. S. Haldane at Oxford) as head of the Department of Physiology, in 1922 of D. Wright Wilson (of Yale and Johns Hopkins) as head of the Department of Physiological Chemistry, and in 1926 of Eliot R. Clark (of Johns Hopkins) as head of the Department of Anatomy. Funds were raised for a new Anatomy-Chemistry wing for the Medical Laboratories and for the first time adequate modern quarters became available for research and teaching in these disciplines.

In 1925 Richards and his friend William Pepper were given honorary degrees (Sc.D.) by the University of Pennsylvania. For Richards, this was the first of thirteen such honors. The next year (1926-1927) he was granted sabbatical leave to accept an invitation from Henry Dale (now Sir Henry) to join in an exploration of the depressor (vasodilator) action of adrenaline. During part of this year he joined his former colleague Cecil Drinker in Krogh's Laboratory of Physiology in Copenhagen, where exciting investigations were being made of the functions of capillaries. While there he performed the simple experiments that proved beyond question the secretion of phenol red by the tubules of the excised kidney of the frog. Later in the year he made a similar experiment on slices of rabbit kidney in Dale's laboratory in London and got a similar result.

In 1927 Richards was elected to membership in the National Academy of Sciences, of which he was to become President twenty years later. By 1927 Arthur Walker was developing into a tower of strength in the kidney research project and was leading a group of able, eager, and devoted young assistants in quantitative chemical studies on fluid collected at various parts of the nephrons of frogs, snakes, and necturi. Reports of these studies were beginning to attract world-wide attention. The teaching program continued to appeal to the medical students and recent graduates were turning in increasing numbers to Richards' department for an experience in research and teaching in a basic medical science before settling down to a clinical career. This was before the development of specific residency training programs and medical graduates had great elasticity in the use of the years between the internship and the final selection of a career. Such young men contributed enormously to Richards' research and teaching programs and further raised the esteem in which his department was held.

In 1931 a new and relatively small private research foundation, the Commonwealth Fund, began a ten-year program of substantial financial support for Richards' kidney research projects. Simultaneously, new quarters were made available by the moving of the Department of Research Medicine from the Medical Laboratories Building to a newly opened Maloney Clinic Wing of the University Hospital. Richards was assigned the vacated space, which his new support enabled him to modernize. Thus he had adequate quarters and funding for the first time in his life. He delegated responsibility for the teaching program to me and proceeded to concentrate on the kidney research enterprise.

The years 1931-1939 were the Golden Age of his research career. Arthur Walker was the leader of a small permanent staff (Walker, Barnwell, Westfall, Bott, Kempton) who were supplemented by a succession of able young men from all over the world. The list includes Howard Florey, Leonard Bayliss, André Simonart, Charles Hudson, James Hendrix, Thomas Findley, James Bordley, John Brown, Frank Queen, Earl Wood, Hubert Royster, Julius Comroe, Eugene Landis, and Robert Dripps, as well as the earlier accessions (Starr, Hayman, Montgomery, Mendenhall, Livingston, Elsom, and Abbott). Not all of these spent all their time on the kidney research project, but there were frequent contacts and discussions, and affection for Richards was a compelling bond.

By 1932 the new enterprise was in full operation and the techniques became more and more sophisticated. Richards had a personal hand in all the activities, but a large share of credit must be given to Walker's doggedness, dexterity, and devotion. A major new development was a procedure for introducing a substance at one level of a kidney tubule and recovering it from another. One of the most pressing questions had to do with the location and extent of the absorption of water by the tubular walls. To obtain pertinent evidence Richards sought a harmless substance which would not diffuse through the tubular walls when so introduced. Such a substance, injected in known concentration into one part of a tubule, could be withdrawn from a lower segment, and any change in its concentration would be a measure of the transport of water into or out of the tubule. He tried dilute starch paste and found, to his surprise, that it underwent partial digestion during its stay in the tubule, presumably because of the presence of starch-splitting enzymes. Casting about for another polysaccharide that would not be digested, he recalled his experience with inulin in Hiss's laboratory at Columbia thirty years previously. He procured a sample of pure inulin from the Bureau of Standards in Washington, tried it, and found it exactly what he wanted.

Further work with inulin showed that it was filtered by the glomeruli, was not reabsorbed in the tubules, was not altered by kidney tissue, was not excreted in the urine of the toadfish (which has no glomeruli), but was rapidly eliminated in the urine of the dog after intravenous injection. Thereupon Richards conceived the idea that inulin might be used to measure the rate of glomerular filtration in mammals and perhaps in man. Accordingly in June 1933 he (with Westfall and Bott) undertook a series of comparisons of the renal excretion of inulin and creatinine (which is filtered by the glomeruli and not reabsorbed by the tubules) and in 1934 a corresponding study was made with xylose. The results indicated that inulin behaves like creatinine in the dog and can be used to measure glomerular filtration, whereas xylose is less satisfactory. This was the extent of the use of inulin to measure glomerular filtration in Richards' laboratory. The subsequent clinical applications came from Homer Smith's group, who apparently conceived the same idea at about the same time and developed it quite independently.

Richards now was world-famous and honors came to him correspondingly. His initial Sc.D. from Pennsylvania (1925) was followed by the same degree from Western Reserve (1931), Yale (1933), Harvard (1940), Columbia (1942), Williams (1943), Princeton (1946), New York University (1955) and finally, in 1960, from the Rockefeller University, sixty years after he received for the support of his youthful research the first grant to be made by what was then the newly created Rockefeller Institute for Medical Research. He received an LL.D. from Edinburgh in 1935 and from Johns Hopkins in 1949, an honorary M.D. from Pennsylvania in 1932 and Louvain in 1949. He was awarded the Gerhard Medal by the Philadelphia Pathological Society in 1932, the Kober Medal by the Association of American Physicians in 1933, the Keyes Medal by the American Association of Genito-Urinary Surgeons in 1933, the Gold Medal of the New York Academy of Medicine in 1936, the Lasker Award in 1946, and the Kovalenko Medal of the National Academy of Sciences in 1953. In 1938 he was invited to give the Croonian Lecture for the Royal Society of London and in 1942 he was elected a foreign member in that Society. In 1934 he received the John Scott Medal of the City of Philadelphia and in 1937 he was given the annual Philadelphia Bok Award. He was awarded the Medal for Merit of the U.S. government in 1946 and was made an Honorary Commander of the Order of the British Empire (C.B.E.) in 1948.

Meanwhile he became increasingly involved in administrative duties at Pennsylvania, and his influence progressed as Pennsylvania developed into a leader in medical research. In 1939 he was appointed Vice President in Charge of Medical Affairs, a position which he held until his retirement for reasons of age in 1948. His predecessor—Alfred Stengel, Professor of Medicine at Pennsylvania—had been able to secure funds to construct a new addition to the medical facilities of the University Hospital and to support the staffing and equipping in it of the Johnson Foundation for Medical Physics, the Cox Institute for Metabolic Research, and a new Laboratory of Research Medicine. Richards worked with characteristic diligence and effectiveness to perform a similar service for the surgical end of the hospital and he succeeded to the extent of a new Agnew-Dulles Wing dedicated largely to research.

The new duties forced him to curtail drastically his participation in the kidney research project, which now entered what proved to be a final phase of work (led by Walker) on the mammalian kidney. Early in 1941 Richards was invited to come to Washington as chairman of the new Committee on Medical Research of the Office of Scientific Research and Development, an organization intended to enlist the cooperation of American scientists in World War II. He was given leave of absence from Pennsylvania and began his new duties on July 15, 1941. He proceeded to divide his time between Philadelphia and Washington until December 31, 1946, when the Washington organization was abolished and he resumed full-time service at Pennsylvania. In the summer of 1941 Walker finished the work under way in Richards' kidney research project and subsequently joined Richards in Washington as an executive assistant. The quarters occupied by the kidney research group were turned over to a new project in aviation medicine, of which the Department of Pharmacology was the center, and were never used again for the original purpose.

From mid-July 1941 until the end of 1946, Richards spent the major portion of his time and energy in Washington, but he usually managed to find a day or two in each week for vice presidential activities at Pennsylvania. One of the most important of these turned out to be the organization of a new Department of Anesthesiology under Robert Dripps. This was a joint undertaking of Pharmacology (whose staff Dripps joined in 1938) and Surgery (of which I. S. Ravdin had recently become the head), but it was consummated by Richards' personal efforts. Dripps's department has gone on to add new distinction to the University of Pennsylvania and to provide another enduring memorial to Richards' term as Vice President in Charge of Medical Affairs.

In his Washington career Richards displayed the same qualities that had won him respect, admiration, and devotion in all his previous endeavors, and with the same result. Under his personal guidance, his Committee on Medical Research secured the cooperation of American medical science in a variety of major research enterprises of vital importance to the war effort. Owing to the foresight of Richards and his associates, the organization was fully functioning when the

Japanese attacked Pearl Harbor and actual war-directed work got under way with much less than the usual delay. The activities involved practically all facets of medical science, clinical and laboratory, and they enlisted the cooperation of academic, governmental, and industrial institutions. Those activities to which major attention was devoted included aviation medicine, trauma, burns, blood substitutes, tropical diseases, bacterial chemotherapy, shock, aerial transport of wounded, and protection against chemical and biological weapons. A comprehensive program on antimalarial drugs, originally undertaken because of interruption of the supply of quinine, led to the development of synthetic drugs far superior to quinine in the prevention and treatment of malaria. The blood substitute program made possible the saving of many lives that would otherwise have been lost. In aviation medicine there were great improvements in protective equipment and a number of advances in fundamental science which paved the way for the postwar developments of the Space Age. Perhaps the greatest immediate contribution to human welfare was the development of penicillin from a laboratory curiosity to a practical therapeutic agent.

In this development Richards played a key role. His own version of it appears in his last scientific publication (1964). Written with characteristic self-deprecation, the paper makes no mention of his personal contribution to the decision to concentrate attention on the production of penicillin by natural fermentation rather than by synthesis. However, I clearly remember his telling me, shortly after Howard Florey came to the United States in the summer of 1941 to start the entire series of events, of the difficult decision between natural production by fermentation (which then was so inefficient as to preclude any but a token supply of penicillin) and laboratory synthesis (which had never been accomplished but which expert chemists then regarded as quite feasible). True to his lifelong adherence to the principle of going from the known to the unknown one step at a time, he used his influence in favor of natural production, which was known to have been successful, though so inefficient that as much as 2,000 liters of culture medium had to be processed to treat a single case of sepsis.

This turned out to be an inspired decision. Within two years, improvements in the culture medium and the selection and development of more productive strains of the mold increased the yield of penicillin from one unit to 900 units per milliliter of culture medium. In 1943 production in deep vats was begun and by June 1944 there was enough penicillin from American and British sources to treat the casualties of the Normandy invasion. At the time of the Japanese surrender in August 1945, production in the United States had risen to 650 billion units per month. By this time British and American chemists had succeeded in synthesizing minute amounts of authentic penicillin, but the cost of producing 100,000 units by natural fermentation already was less than the cost of the labor and material required to put it into an ampoule, and there was no prospect of economic advantage from synthesis. The same situation still prevails.

The activities of Richards' Committee on Medical Research were gradually terminated during 1946 and the organization was abolished at the end of that year. He returned to his post as Vice President in Charge of Medical Affairs at Pennsylvania and served in this capacity until June 1948, when he retired and became Emeritus Professor of Pharmacology. His senior associate in the kidney research project, Arthur Walker, did not return to Philadelphia and Richards made no attempt to resume the project. During these last years of his active career he received three more honorary degrees in addition to the Medal for Merit of the U.S. government and the Order of the British Empire.

In 1947 Richards was elected President of the National Academy of Sciences. For the duties of that high office he was well qualified by his previous service as chairman of the Committee on Medical Research of the OSRD. During the war years, he had worked in the house of the Academy and had there gained a broad understanding of how the Academy and its Research Council could further science and the national welfare through close relations with many sections of the federal government.

The chairman of the National Research Council during his presidency was Detlev Bronk, his friend and Pennsylvania colleague of many years. To him Richards gave strong support, encouragement, and freedom in the development of the Council as scientific adviser to the government and to private foundations. During his presidency of the Academy his wise counsel and sound judgment were widely sought by congressional committees and executive agencies.

Under his vigorous leadership the scope and significance of the Academy's Research Council quickly grew. As it did so, and as he envisioned an ever-growing role for science in government and universities, Richards modestly decided to retire in favor of a younger man. That he did in 1950. In 1948 he was appointed to membership in the Medical Section of the First Hoover Commission on the Organization of the Executive Branch of the U.S. government and was active in this enterprise until March 1948. During the same year he became a member of the Board of Directors of Merck and Company, to whom he had already been a consultant for some twenty years. This relationship was a major source of satisfaction in the final stage of his career. The nature of the relation is indicated by his selection to serve as chairman of the Scientific Committee of the Board of Directors during the 1953-1955 period. From 1948 until his death he also was an Associate Trustee of the University of Pennsylvania.

Final honors were the presentation of the Abraham Flexner Award of the Association of American Medical Colleges in 1959, for outstanding service in the teaching of medical students, and the naming after him in 1960 of a new medical research building attached to the Medical Laboratories which had been his scientific home for the preceding fifty years. His remarks on that occasion are recorded in *Medical Affairs,* a publication of the Pennsylvania Medical School, in the summer of 1960. They include a revealing summary of his attitude toward life in general and toward the University of Pennsylvania in particular. His words were as follows:

"Any name that is carved into a permanent structure of this, the oldest and, as we choose to think, the most illustrious school of medicine in this country, acquires an incomparable distinction. It still seems incredibly unreal that the name which is being thus recognized today should be the one my parents gave to me and I can expect that the rest of my days will be periodically beset with alternating periods of elation, doubt, and disbelief—never, I am sure, without humility.

"This latest indebtedness to the University of Pennsylvania about to be incurred today is the culmination of a long series of blessings which have come through my half-century of associations here. It seems not only appropriate but obligatory that I recite those which have meant most to me."

He went on to enumerate the generous arrangements which Provost C. C. Harrison made for the start of his career at Pennsylvania in 1910; the leaves of absence granted him by Provosts Edgar F. Smith in 1917 and Charles Penniman in 1926 to enable him to work with British physiologists; the addition to his budget in 1921 of funds which made it possible to add young physicians to his staff and thus to develop a program in clinical pharmacology; the authorization to accept the support of the Commonwealth Fund for the years 1931-1941; a leave of absence for five years beginning in 1941 to accept a presidential appointment for war work in the Committee of Medical Research; the privileges of a laboratory and equipment which enabled him to do what he most wanted to do, with small administrative duties; a dean, Dr. William Pepper, who for thirty-two years gave him friendship, encouragement, and the help of ripe wisdom and calm judgment; and younger associates through whose independent intelligence and skill important progress was made.

He acknowledged the generous financial contributions of the U.S. Public Health Service to the construction of the building that now bears his name, and to the support of the investigators working in it. Then he had this to say:

"We are proud that our investigators have the quality which warrants the confidence and support of the granting agencies and their expert advisors. We do not now fear the old imagined threat of federal control. But ready access to abundant research funds is associated with subtle temptations not easy to recognize and resist.

"Those are the temptations to ask for more than is needed or justified; to overestimate the influence of volume of results perhaps at the expense of quality; to delegate too much of the work to assistants or technicians and thus for the investigator to be deprived of opportunity of seeing the chance breaks which nature so frequently yields. There is also the temptation to shape the plan of a research to suit what are believed to be the aims of the granting agency or to the peculiarities of the expert advisors; the temptation to forget that a reasonable degree of austerity is a wholesome stimulant; that excessively elaborate equipment can convert the investigator into a technologist and may obstruct his direct vision of the living system which he studies and of its beauties; that discoveries are usually made not by teams, but by an individual, lonely and tormented by a passion to break through the blank wall which stands in the way of his understanding."

This, his last public utterance, seems to me to contain an appropriate *apologia pro vita sua.*

His last years were clouded by the slow advance of an inoperable neoplasm, and by the tragic death of his son and only child in an airplane crash in November 1962. Death came to him from a respiratory infection on March 24, 1966, two days after his ninetieth birthday.

In the preceding pages I have attempted to present an impersonal account of the career of Alfred Newton Richards. The information on the period prior to 1919 (when my own association with him began) is derived for the most part from copious notes left by him for the benefit of his biographer, a circumstance that bespeaks the sympathetic consideration for others that was one of his outstanding traits. For the rest, I have depended principally on my own memory, supplemented by his own notes and publications and by comments from other former colleagues and friends. The bare facts leave little doubt that this was one of the most distinguished and important medical scientists of his generation. Having been associated with him as student and colleague for fifty years, I am quite certain that that is how he would want his biography to end. I am equally certain, however, that the importance of this man depended more on the impact of his personality on his contemporaries than on his scientific achievements, great as these were. Actually the two were so interwoven that a meaningful evaluation of his career requires an attempt to add the contours of a warm human being to the bare skeleton of his accomplishments. Therefore I venture to append a brief account of the part played by his personal traits, as I knew them.

His career seems to have been determined by a series of apparently coincidental openings of doors whose existence must have been previously unknown to him. The coincidences were too numerous and too uniformly fortunate to be entirely accidental, and those who had the good fortune of working with him will have no difficulty in discerning the imprint of the character traits that attracted and held us to him. The most important of these were complete honesty (coupled with an extraordinary capacity for making himself the most severe critic of his own thoughts and actions), an all-pervading striving toward perfection in everything he undertook (coupled with a surprising degree of physical stamina and a great willingness to draw upon it), high intelligence (along with an abhorrence of pretentiousness and egotism), unselfishness, deep concern for others, a constant search for simple, direct approaches and for evidence that will speak for itself, and a rare, gentle sense of humor.

It is reasonable to suppose that most if not all of these qualities must have been as evident to Chittenden, Hiss, Prudden, Herter, Wallace, Howland, Edsall, and others who opened doors for him at critical periods in the formative stages of his career as they were subsequently to William Pepper, Cecil Drinker, scores of the colleagues and thousands of students at Pennsylvania, Sir Henry Dale and others in England, Vannevar Bush in the OSRD, the Research Council and membership of the National Academy of Sciences, the trustees of the University of Pennsylvania and of the Rockefeller Foundation, the directors of Merck and Company, the faculties of the thirteen universities who gave honorary degrees to him, and the various organizations that presented him with medals and other awards.

He led by example rather than precept and he asked of his associates nothing that he was not ready and able to do himself. When demands on his time and energy came to exceed the possibilities of effective personal participation he did not attempt to maintain a tenuous involvement in all the current activities, but withdrew completely from some of them and confined his efforts to a residue in which personal participation was still possible. Those who came to his department expecting to have the experience of working with him had one reason to be grateful for this policy. Those to whom complete responsibility for some of the department's activities was delegated had another. The common denominator was a great and growing affection to add to the respect and admiration this man already had engendered. His genuine concern for students and colleagues and his legendary sense of humor were additional reasons for strong emotional attachments.

Such considerations do not appear in the record of his career but they were of tremendous importance in generating and maintaining the reputation which attracted to him the younger associates who contributed greatly to his success. In the critical period before his research made him famous his teaching program for medical students was the source of attraction. From firsthand experience I know the incessant search for improvement and the enormous expenditure of physical and mental effort that underlay the favorable reaction of the students. He used to say that the sole reason for the existence of a teaching department in a medical school is to teach medical students, that research is a privilege and not an inalienable right. Thirty years before the project method of medical education became popular in America he used to encourage his younger associates to incorporate their current research activities into the student laboratory program, and we did as much of this as our limited resources would permit. Eventually I realized that in so doing we were simultaneously maximizing the interest of the students (by letting them in on the formative stages of the solution of problems) and that of the younger staff members (by giving them responsibility for demonstrating the importance of their own brain-children). This interplay was a major part of the attraction of his laboratory for medical graduates.

In the account of his career I have tried to show how his success in research was directly due to his unremitting search for improvement in his teaching. Those of his associates who went on to teach medical students followed his example, with similar results. My own efforts along those lines underlay the discovery of ephedrine (which resulted from the injection of a crude extract of an unknown Chinese drug in the course of a practice experiment in preparation for a student exercise) as well as my interest in respiratory control and the cerebral circulation (which was part of a lifelong search for an explanation of the effects of morphine on respiration).

His unremitting efforts to improve his teaching of medical students were largely responsible for his rise from obscurity to international fame at Pennsylvania. He came there largely because the University had decided to follow the current trend toward the physiological approach to pharmacology and Richards had already organized and conducted successful student courses at Columbia and Northwestern. His recent contributions to the understanding of delayed chloroform poisoning, together with his services as assistant editor of the *Journal of Biological Chemistry* and the favorable reports on his teaching of medical students, must have marked him as one of the outstanding young pharmacologists of his day. It is noteworthy that he began his career as a biochemist and was led to become a pharmacologist by a series of coincidences involving his introduction by Hiss to Prudden and by Prudden to Herter, shortly before Herter undertook to organize a department of pharmacology at Columbia. The arrival of George Wallace and the strong friendship and admiration that soon developed between Richards and Wallace represented another coincidence. So did the establishment of the *Journal of Biological Chemistry* with Herter as editor while Richards was working with him.

But it was the unbroken record of superior performance of teaching duties that made possible the events that brought him fame. In his subsequent career at Washington he made an entirely new set of admirers and friends and from conversations with some of these I know that the same character traits were responsible. The performance was repeated in his relations with Merck and Company after his retirement.

Thus from the start of his career to its close his impact on his environment went from obscurity through uncertainty to interest, admiration, and affection. Many anecdotes attest to his wisdom and personal charm. Those of us who knew him well will always remember the infectious grin and chuckle that characterized his moments of relaxation as well as the example of modesty, self-criticism, and search for perfection that he set us. Some of his character traits may have been derived from his parents and teachers, but some were all his own. It was a unique combination.

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KEY TO ABBREVIATIONS

Am. J. Med. Sci. = American Journal of Medical Sciences
Am. J. Physiol. = American Journal of Physiology
J. Biol. Chem. = Journal of Biological Chemistry
J. Pharmacol. Exp. Therap. = Journal of Pharmacology and Experimental Therapeutics
J. Physiol. = Journal of Physiology
Penn. Gaz. = Pennsylvania Gazette
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OSWALD HOPE ROBERTSON

June 2, 1886–March 23, 1966

BY L. T. COGGESHALL

DR. OSWALD HOPE ROBERTSON was born in Woolwich, England, June 2, 1886. When he was one and a half years of age, his parents moved to California and settled in the San Joaquin Valley at a time when it was still in a very primitive condition. He loved the out-of-doors, especially the small animal life of the valley and surrounding mountains.

Robertson began and ended his career as a naturalist in this same environment. In the intervening years his contributions to medical science were highly regarded by his colleagues, not only for their basic importance, but also—and especially—for the convincing array of indisputable evidence to support his contentions. He was ingenious in designing apparatus to meet the needs of his experiments. For example, in the early thirties he designed a heart-lung machine to induce artificial pneumonia *in vitro*. It was a masterpiece and almost completely successful; all that was lacking was the discovery of latter-day electronics that has made modern scientific and therapeutic equipment so spectacular.

He was an able clinician as well as an extremely effective teacher, especially with small groups. Occasionally he assumed important administrative assignments but the restraint of his enthusiasm for those responsibilities was quite obvious.

After attending the local primary schools in Dinuba, California, Robertson graduated from Polytechnic High School in San Francisco.

Originally he planned a career in basic biology but was diverted from this course while on a vacation in Germany. Here he met an American medical student and was invited to accompany him to some lectures and demonstrations in anatomy and clinical medicine. This brief insight into the realm of human biology and pathology provided a new direction to his future course. He decided to study medicine and was admitted on his return to the premedical course at the University of California in 1906. His research instincts asserted themselves early. Even as a first-year student, he did some creditable research on the complement fixation test of rabies in the laboratory of F. P. Gay. Later in the same laboratory he worked on a diagnostic test for glanders. Although these studies were never published, they were written up and accepted for a Master of Science degree in 1910. At the instigation of friends he transferred to Harvard Medical School as a junior student. The high quality of his performance in medical school won for him the prized Dalton Scholarship Award to study pernicious anemia at the Massachusetts General Hospital, where he was appointed as an intern in 1913-1914. During this busy period, he had his first paper published in *California and Western Medicine* on Ehrlich's test for urobilinogen as an index of liver function. By this time his career as an investigator was established and he accepted a renewal of the fellowship to pursue his interests in the experimental and clinical manifestations of certain blood dyscrasias.

After his house officership he was appointed as an assistant in bacteriology and pathology at the Rockefeller Institute for Medical Research, where he planned to continue his studies on the physiology of blood in the laboratories of Dr. Peyton Rous. He and his new bride, Ruth Allen, a nurse he met at the Boston Children's Hospital, moved to New York in the autumn of

1915; but he had barely started his experiments on reticulocytes when World War I called for his services on the Harvard team of Dr. Harvey Cushing. In France, his early interests in hematology stood him in good stead as he was assigned the task of seeking better ways of reducing the risks of excessive transfusions. At the same time he initiated experiments to learn whether a fluid devised by Rous in the laboratory to preserve human blood cells *in vitro* had a practical use for transfusions at the battle front and in military hospitals. He demonstrated that the preserved cells were indeed an acceptable substitute, and Robertson now is recognized as the creator of the first blood bank. For this work he received decorations from both the American and British governments and was discharged in 1919 from the U.S. Army after attaining the rank of major.

He returned to the Rockefeller Institute and began his important studies in the field of infectious diseases, first with Hideyo Noguchi and then with O. T. Avery. In Avery's laboratory he became interested in the pneumococcus and pneumonia, which engaged his attention for more than twenty years. However, again his studies were interrupted by an attractive offer he accepted to assist in the development of the newly established Peking Union Medical College. He was appointed Associate Professor of Medicine and in 1923 he became Professor of Medicine and head of the department. He loved the eight years spent in China, a period of intense clinical activity and a reactivation of research interests in the infectious disease group—particularly in the pneumococcal group. Robertson, working closely with the young Chinese students, as well as with the staff, was given much credit for the successful growth of the institution. It became a hallmark for medical excellence in the Oriental world. The patterns of modern Western methods were accepted and established so readily that repeated efforts to negate them for political reasons were unsuccessful. Although his professional activities kept him very busy he still found time to roam the countryside, to hunt bustards, or just to observe the native fauna and flora. He acquired a host of Chinese friends who made his home and laboratory a favorite meeting place—a practice they continued when visiting him after the Robertsons returned to the United States in 1927 following his recovery from an attack of typhus fever.

After a lengthy convalescence he accepted a professorship in medicine at the University of Chicago, where university-owned hospitals had just been built on the campus. He was intrigued by the opportunity to become associated with the group objectives, which were unique in American medical education and care in three important respects: (1) it was the first time an entire faculty was on a full-time basis as university faculty members without any private practice and remuneration; (2) all patients were utilized for teaching purposes; and (3) the clinical faculty were combined with the nonclinical group into a single division of biological sciences. As these plans became operative in the late twenties they met considerable resistance, but after four decades they are now accepted as sound and have become models for many other institutions, both existing and new ones. Robertson's role during the early growth pains was not insignificant. For a time he was the able administrative head of medicine, but this responsibility he transferred as soon as other competent hands could be found. In the research laboratories his studies on the pathogenesis and immunology of lobar pneumonia were classic. It was at this time he developed the heart-lung machine with his group, of which it was a privilege to be a member.

The practical application of his basic studies on pneumonia was an effective antipneumococcal serum. It was the first specific therapy against the dreaded lobar pneumococcal pneumonia and enjoyed only a brief period of success, as the new sulfa drugs, which had much more rapid action and were far less expensive, were soon discovered.

Beginning in 1939 under Robertson's leadership a laboratory group directed their efforts toward elucidating the factors of significance in air-borne infections and especially to the discovery of techniques for aerial disinfection. These studies made necessary an excursion into many new fields, including physical-chemical studies of aerosol behavior; the epidemiology of respiratory infections in experimental animal populations and in human groups; the toxicology of various chemicals designed to kill air-borne bacteria and viruses; the pathogenesis of bacterial and viral infections transmitted via the aerial route; and the design and testing of engineering equipment for application in the field of some of the measures that were devised in the laboratory. In the period which followed, the laboratory group expanded and included the following persons: Edward Bigg, Morton Hamburger, Henry Lemon, Clayton Loosli, Benjamin and Zelma Miller, Theodore Puck, Henry Wise, and William Lester. In 1942 Robertson was named the director of the Commission on Cross Infections in Hospitals of the U.S. Army Surgeon General's Office. The name of this commission was later changed to the Commission on Air-Borne Infections.

Dr. Robertson's laboratory then undertook a double program of field studies in hospitals, army barracks, and industrial installations and laboratory experiments in an effort to develop methods of combating the spread of air-borne infections, which had become an exceedingly important source of attrition of human effectiveness during the war. Studies were carried out on the physical chemistry of aerosols containing viable infectious agents. Effects of various parameters on the viability of these agents were studied and the mechanism of air sterilization by chemical agents was worked out in detail. Such studies showed that the previously held theory that bactericidal action was exerted through collision of aerosol particles of germicide with aerosol particles of the infectious agent was erroneous, and that the actual mechanism of action required action of the bactericidal agent through the vapor phase. New agents were developed which were enormously more effective than previous materials, and which were shown to be without toxic effects to human populations. The physical chemistry of aerosol vapor interactions was studied, and a number of fundamental papers were published in this field. While the use of chemical air sterilization was developed to the point where it became a practical measure, it never proved possible to obtain clear-cut epidemiological data demonstrating beyond any question that these measures would reduce respiratory disease in ordinary situations of human habitation. Nevertheless, the fundamental advances obtained in the course of these studies have been useful in a variety of ways, in both pure and applied science.

The hectic pace of the scientist with important contributions to make to the military effort during World War II held little appeal to Robertson and after the peace treaties were signed he requested an early retirement from the University of Chicago. He had a nostalgia with which most native Californians are afflicted and in 1950 he moved to the West Coast, where he established a laboratory in the Santa Cruz mountains. Here he began another successful research period. To the amazement and envy of most of his colleagues, salmon and trout occupied his attention. The problem was the cause of death of all Pacific salmon shortly after spawning, an almost unique phenomenon in the vertebrate world. Characteristically he began with the basic ingredients—a study of piscatorial anatomy and physiology during the migratory period. He found marked changes in the fatal journey of the salmon to the spawning grounds. The pituitary glands became hyperactive as their adrenals become overstimulated. This created a condition similar to Cushing's syndrome in human beings. The destructive alteration of function and structure was directly related to the hyperconcentration of cortisone and hydrocortisone in the blood, plus the starvation which the salmon undergoes on its final voyage.

His last studies, terminated by death in 1966, were attempts to create artificially the most amazing natural phenomenon by injecting hormones in the immature rainbow trout; also, he tried to prevent or postpone degeneration and post-spawning death through the use of adrenal-suppressing compounds.

Robertson became a member of the National Academy of Sciences in 1943.

During his California period Stanford University was of great assistance and made him a member of its faculty. He quite naturally joined the ranks of the zoologists and was accepted as he was in medicine, a top-quality scientist. As one of his closest and most respected friends, Peyton Rous, said, "During recent years, working as a medical scientist still, you have revealed biological truths of such magnitude that their implications for natural phenomena have yet to be realized." He said this as he presented Robertson the Kober Medal, the most illustrious ornament medicine has to offer to one of its scientists.

The scientists working in his laboratory never failed to be deeply and permanently affected by his example of courtesy and gentleness in dealing with people, which reflected deep inner conviction about the worth of every human being. It was a joy to see how he manipulated scientific problems in order to reduce them to relatively simple questions, each of which could be approached by laboratory experiments. He was a masterful experimenter who designed techniques which were deceptively simple. This simplicity, however, resulted from his deep consideration of the essential process underlying the phenomena studied. This analysis enabled him to eliminate many of the disturbing processes so as to isolate the single element which his subsequent experimentation was designed to elucidate. He taught young men by example, by gentle criticism, and by generous approbation.

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KEY TO ABBREVIATIONS

Am. J. Hyg. = American Journal of Hygiene
Am. J. Med. = American Journal of Medicine
Am. J. Med. Sci. = American Journal of Medical Sciences
Am. J. Public Health = American Journal of Public Health
Am. Phil. Soc. Year Book = American Philosophical Society Year Book
Arch. Internal Med. = Archives of Internal Medicine
Brit. Med. J. = British Medical Journal
Gen. Comp. Endocrinol. = General and Comparative Endocrinology
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Clin. Invest. = Journal of Clinical Investigation
J. Exp. Med. = Journal of Experimental Medicine
J. Immunol. = Journal of Immunology
J. Infect. Diseases = Journal of Infectious Diseases
J. Morphol. = Journal of Morphology
Physiol. Zool. = Physiological Zoology
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
Trans. Assoc. Am. Physicians = Transactions of the Association of American Physicians

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John H. Williams

JOHN HARRY WILLIAMS

July 7, 1908–April 18, 1966

BY ALFRED O. C. NIER

JOHN HARRY WILLIAMS was born in Asbestos Mines, Quebec. His father, who was a mining engineer and who served in the Canadian Army, died in World War I. John and his three brothers were raised by their widowed mother in the small town of Kelowna, British Columbia. Here he attended the public grade and high schools and had the good fortune to be stimulated by a superior teacher of science and mathematics, with the result that his talents in these areas readily became apparent. A scholarship enabled him to attend the University of British Columbia, from which he graduated with a major in physics in 1928.

His lifelong interest in sports and the outdoors dates back to his childhood, when swimming, hockey, camping, fishing, and hunting were natural activities along with his chores in the family orchard. In his senior year at the University of British Columbia he married Vera Martin, a teammate on the swimming team. Three children, Lloyd, Margaret Ann, and Susan, were born to the couple.

Willaims attributed his resolve to pursue graduate work to the chance he had to observe and learn from dedicated teachers in both high school and university. In 1928 he began his graduate studies at the University of California, Berkeley, and received his Ph.D. degree three years later. Because of his excellent undergraduate training, his research, under the direction of Professor Samuel K. Allison, began immediately upon entrance to the Graduate School. This led to a paper on a double crystal x-ray spectrometer by the end of the year. At this time Allison left Berkeley to accept a position at the University of Chicago. Williams completed his thesis work, corresponding with Allison as necessary. In 1931, when he received a National Research Council Postdoctoral Fellowship, he followed Allison to Chicago, where he spent two more profitable years that resulted in important publications on the widths and intensities of x-ray lines.

The summer of 1933 was spent as a writer for Science Service in Washington, D.C. In the fall he accepted a position as research assistant to Professor John T. Tate at the University of Minnesota. Here he joined in work on the ionization and dissociation of gases, for which the Minnesota laboratory had become famous, but his stay in the field was brief. This was the time when the frontiers of nuclear physics were being opened, so it was only natural that the resourceful and energetic John Williams, with the active encouragement of John Tate, should start a program in the exciting new field.

The laboratory already owned a 275-kev transformer-kenetron-condenser source of high voltage, and this was soon supplemented by an ion source, an ion accelerating tube, target chamber, counters, and pumping systems. Much of the equipment was constructed personally by Williams, for this was in a day when funds for research were limited. In his almost overwhelming task, Williams was soon joined by William H. Wells, a postdoctoral fellow, and several graduate students who began their distinguished careers under Williams' tutelage. The program prospered, and significant papers on disintegration processes in the light elements appeared.

A year after arriving at Minnesota, he was appointed as an instructor and began the progression through the academic ranks. By 1937 he was promoted to an associate professorship, a remarkable achievement since promotions in those dark depression days were very limited at Minnesota.

Neither Williams nor Tate was satisfied with the limited energies provided by their nuclear equipment, so work was started on a Van de Graaff generator which it was hoped would give energies up to one million electron volts. This instrument was never completed, because it became apparent that the University ought to embark on a much more ambitious program. It should be remarked that this was in a day when faculty were not generally relieved from teaching duties to perform research, so Williams, in addition to working actively to build a nuclear physics program—to a large extent with his own hands—carried a full teaching load which amounted to lecturing in two full courses plus teaching an advanced laboratory for part of the year.

During 1936 it was decided to construct a pressure Van de Graaff generator which would provide energies of at least 3 million electron volts. The cost was beyond the resources of the institution, so foundation help was sought. In the 1930s funds for pure physics research were practically unavailable, but there was a growing appreciation of the importance of nuclear physics research to biology and medicine, through providing either radioactive isotopes for tracer studies or energetic particles for investigation of the effects of such particles on biological materials. Accordingly, a committee of outstanding researchers in the University's biological, medical, and chemical departments, headed by John Tate of the Physics Department, and augmented by a strong group from the Mayo Foundation for Medical Education and Research (a division of the University Graduate School), approached the Rockefeller Foundation for a grant of $36,000 to build a Van de Graaff generator and to finance a program of interdisciplinary research using this facility.

At their meeting on April 7, 1937, the trustees of the Rockefeller Foundation approved the request and the project was launched. It is interesting to note that while the request was made in the name of a prestigious group of senior scientists in a variety of fields, the key man in the picture was the twenty-eight-year-old John Williams, who would have the responsibility for the design and construction of the machine. In the fall of 1937 he was joined by Lynn H. Rumbaugh, another very promising young faculty member. Until World War II intervened, they shared the responsibility for getting the new major facility into operation. Again, it should be stressed that much of the effort involved donning coveralls and helping with the actual construction and adjustment of the equipment. This firsthand experience was to stand Williams in good stead later when, during World War II, he had charge of the Van de Graaff work at Los Alamos and when, in his subsequent associations with the Atomic Energy Commission, he was called upon to evaluate difficult research problems in a wide variety of fields.

As the war clouds gathered in 1940, faculty members were leaving their posts to accept defense-related positions, and Rumbaugh left to go to the Naval Ordnance Laboratory. Tate had already gone to head Division 6 of the Office of Scientific Research and Development, so Williams, together with a full-time assistant and several graduate students, was left alone to administer and complete the construction of the generator. During early 1940 the first ion beam was obtained, and shortly thereafter actual experiments were undertaken. Because he was not yet a citizen, he was not able to work on defense-related research, a source of great regret to him. When he obtained his citizenship on January 19, 1942, he immediately undertook a program of neutron cross section measurements related to the atomic bomb project.

In March 1943, he, together with several students and colleagues, left for Los Alamos, where he was placed in charge of the two University of Wisconsin Van de Graaff generators which had been moved there for the purpose of making a variety of nuclear cross section measurements related to the design of atomic bombs and reactors. In 1945 he served as deputy director for the first atomic bomb test, and as the one in charge of the Services Division assumed responsibility for the wiring, power, transportation, communication facilities, and construction for this enormous undertaking. Along with his many scientific and technical responsibilities at Los Alamos he helped organize the school system and served as president of the School Board.

Although very much involved in his Los Alamos work, Williams kept up a constant correspondence with J. W. Buchta, chairman of the Minnesota Physics Department. With the exodus of faculty from the department during the war, research activity had come to a complete halt. Williams was very anxious that after the war the department should be restored and strengthened. The University administration seemed not to understand the profound changes which the war was bringing about, and appeared to assume that once hostilities ended one could merely pick up where one had left off. At Los Alamos Williams had the benefit of close contact with top physicists from all over the United States and gained a deep appreciation of how much the world of physics would be changed as a result of the war.

After helping with the Bikini bomb tests, he returned to Minnesota on April 1, 1946. Together with a group of students, some of whom had worked for him at Los Alamos, he plunged into an active program of remodeling and rebuilding the Minnesota Van de Graaff generator. By 1947 the machine was back in operation, and a long list of graduate students began their research careers under Williams' guidance.

Although the machine continued to produce useful research until 1967, when it was finally retired, Williams anticipated its obsolescence and, almost before it was back in operation after the war, made plans for a higher energy facility. He had a keen sense of the practical and recognized that the best chance for obtaining a new facility lay in presenting a proposal for an accelerator which was appropriate for an institution like the University of Minnesota, and which would produce useful research in an area in which fundamental data were badly lacking. The Atomic Energy Commission recognized the soundness of his reasoning and, in 1949, authorized the construction of a 50-MeV linear proton accelerator at Minnesota. At the time, this was the most powerful accelerator of its type and proved to be an extremely valuable facility for performing new and significant nuclear research. The energy range was later extended to 68 MeV, and at the time of this writing the machine is still in full-time operation.

While Williams had a very real interest in the actual experiments being performed, always taking his turn in shift operations, he was even more concerned that his younger colleagues on the staff, and the graduate students, should have proper facilities so that they might perform significant research and mature as physicists. Among other things, he pushed very actively for the development of adequate machine and electronic shop facilities in the department. The extremely high quality of these facilities in the department today is a fitting memorial to his passion for quality and efficiency. Had he chosen, he could have developed such excellence as a part of the nuclear physics program alone, but he insisted that to provide a healthy climate for all research the shop facilities must be an integral part of the department as a whole, rather than being associated with a particular research program.

The new linear accelerator was placed in operation in the early 1950s. If Williams had been a less energetic man he might have coasted and spent the rest of his days participating in occasional research with the machine, interspersed between the golf and pool games he thoroughly enjoyed. It was at this point that perhaps the most significant part of his career began.

Along with many others, he recognized that one of the most important unexplored areas of physics, if not the most important, lay in the detailed study of elementary particles, using ever higher energy accelerators. The enormous cost of such accelerators made it unlikely that they would be given to any one institution. Both the East and West Coasts of the United States had high energy facilities, but there were none in the Midwest —much to the disadvantage of the institutions in the area. He and colleagues of like mind in other major Midwest institutions convinced their respective institutions that they should create the Midwest Universities Research Association, a nonprofit organization dedicated to the study, design, and eventual construction of a significant high-energy facility in the Midwest. He was chairman of the site-selection committee, which chose an area near Madison, Wisconsin, as the location for the proposed accelerator.

From 1955 to 1958 he served on MURA's Board of Directors and in 1956-1957 was the President of the Association. The MURA Laboratory grew and became a significant research facility, making important contributions to the science and art of accelerator design. MURA members felt that high-energy physics in the Midwest could best be served by the construction of an accelerator under the control of a university group such as MURA, whereas the Atomic Energy Commission felt that the accelerator should be part of the Argonne National Laboratory, an AEC facility operated for the government under a contract with the University of Chicago. The AEC subsequently funded a 12.5-BeV accelerator at Argonne which indeed helped the high energy picture in the Midwest, as well as the nation. However, the MURA group, including Williams, did not feel that this was an adequate facility for the Midwest physicists and continued to push for an accelerator which they felt would be competitive with those in existence or proposed elsewhere.

The controversy highlighted the lack of rapport between the Argonne Laboratory and the Midwest university physicists. To improve the situation a number of positive steps were taken. Along with a number of other scientists, Williams accepted membership on the newly created Argonne National Laboratory Policy Advisory Board, an advisory committee to the Chancellor of the University of Chicago. Positive steps taken by this board included the establishment of visiting committees to the various divisions of the Laboratory, and the general improvement of communications and understanding. In his several roles and relationships with the Atomic Energy Commission, Williams distinguished himself not only as an excellent physicist but also as a fair-minded statesman of science, so that when the position of Director of the AEC's Research Division became vacant in 1958, he was asked to fill it.

Acceptance of the position meant he would have to leave his productive research career, the classes he truly enjoyed, and a comfortable life at Minnesota—for a frantic existence in Washington. Never known as a shrinking violet, he always fought hard for what he thought was right and combined this dynamic character with a deep sense of duty. When asked to accept the AEC position, he explained his feeling of obligation to his country as follows:"Every so often you have to put some chips back in the pot. I put some in in World War II and for the past ten years I have been taking them out. Now I guess the time has come to put more back in."

As Director of the Research Division of the AEC he studied its vast program and made important decisions relative to the country's long-range plans for nuclear research. He traveled widely and ably represented the needs of science to the Congress. In his dealings with the scientific community, he made many new friends and impressed everyone with the fairness and intelligence with which he approached his job.

After filling the position only a year, he was appointed as a Commissioner by President Dwight Eisenhower. He had been in this position only a few months when there was a recurrence of the cancer which had first striken him on July 16, 1951, the anniversary of the Trinity Atomic Bomb Test in which he had played such an important part. He returned to Minnesota in January 1960 for examination and treatment at the University Hospitals, where he remained for some weeks before returning to Washington. Here he received treatments at the Clinical Center of the National Institutes of Health. When he left Minnesota, most of his old friends assumed they would not see him again. Yet, on April 6, 1960, in his first public appearance since the previous December, he was the Commission's mainstay witness in an important congressional hearing concerning authorization of the $114 million Stanford Linear Accelerator Project.

Thanks to medical science and his own fortitude, the illness was controlled, and in the summer of 1960 he resigned from the AEC and returned to Minnesota, weak but filled with enormous courage and determination. He immediately plunged back into teaching and research. The AEC kept him in the "family" as a consultant after his resignation and by fall, when it was apparent that his health had improved, appointed him as a member of the General Advisory Committee to the Commission, a position he held until his death in 1966.

Virtually every year the cancer flared up. Once, it affected his larnyx, and for a long period he could talk in only a hoarse whisper. On another occasion, a hip was afflicted, and there was grave danger that he might be permanently bedridden. Again he recovered, and when he was well enough to leave the hospital, he refused to use a cane and for a long time employed a golf club as a substitute, the head serving as a handle.

In 1959 Williams was elected to the National Academy of Sciences and in 1963 to the presidency of the American Physical Society, an office he conducted with distinction during the term. Although no longer able to lecture he remained extremely active in his many other activities. After he returned to Minnesota in 1960, he again worried about the need for new facilities for nuclear research and, together with younger colleagues, planned for a new accelerator to replace the aging Van de Graaff machine and the linear proton accelerator. His dream came true in 1964 when the Atomic Energy Commission awarded a new 20-MeV "Emperor" Van de Graaff generator to the University of Minnesota. This was housed in a new laboratory building built from funds provided by the state and the National Science Foundation.

Meanwhile, on the national scene, he continued his interest in the health of Midwest physics and in January 1964 was appointed as chairman of a committee to study how the Argonne National Laboratory could be made more effective in carrying out its mission of serving the Midwest scientific community. This committee, appropriately named "the Williams Committee," arrived at the recommendation accepted by the Atomic Energy Commission that there should be a tripartite agreement between a group of universities, the University of Chicago, and the AEC for the management of the Argonne National Laboratory. The new organization of universities, the Argonne Universities' Association, Inc., would have over-all direction of policies and programs for the laboratory, and the University of Chicago would continue to operate the laboratory. In December 1965 Williams was named as the first president of the new association, a position he held until his unexpected death a few months later. Never willing to admit defeat and always anxious to do his share in any enterprise, he plunged into the work of the new association.

The new responsibility included innumerable meetings and weekly trips between Minneapolis and Chicago. It was on one of these journeys that he was stricken with influenza. Upon his return to Minneapolis his condition deteriorated, and a few days later he died of pneumonia. A dedication of the accelerator laboratories, naming them the John H. Williams Laboratory of Nuclear Physics, had been planned for May 3, 1966, but, because of his unexpected death just a few weeks before the proposed celebration, the occasion was changed to a combination memorial and dedication ceremony.

Throughout the years he maintained an interest in sports and the outdoors. He was an ardent golfer until deterioration of his health forced him to give up the game. In 1932, while at Chicago, he, S. K. Allison, and W. H. Zacharaisen started to take biennial fall canoe trips in Quetico Park, Ontario. After World War II it was decided to schedule the trips every year, since "there was less time left." The group was sometimes expanded to six. In spite of failing health, he missed none of the annual trips in the last six years of his life.

He was an active member of the Campus Club (the University of Minnesota Faculty Club) and served two terms on its board of directors. He is the only member in the history of the club to be twice elected to the presidency. As president of the club and later as chairman of the House Committee, he played a very active role in planning an expansion of the club in the years following World War II. While he was a very hard worker, he also believed in relaxation, and throughout his career at Minnesota could usually be found at the club's billiard or pool tables immediately following lunch.

He showed a strong competitive spirit both in work and in play. He was known for his keen sense of humor and thoroughly enjoyed a good story. During his many hospital confinements, he entertained his visitors with the newest jokes he had heard.

Both he and his wife were enthusiastic athletic fans and attended practically all the home games of the University teams in the major as well as the minor sports. He served on numerous all-university committees including, of course, that on intercollegiate athletics.

In 1958 he was awarded an Honorary Doctor of Science degree by his own alma mater, the University of British Columbia, and in 1960 an Honorary Doctor of Engineering degree by the Pennsylvania Military College.

John Williams left his mark wherever he went. He was known as an excellent teacher in both the undergraduate and graduate courses which he taught. His research was recognized for its quality and significance. He was a builder of men and laboratories. He distinguished himself as an administrator and statesman of science, as well as a counselor to those who needed help of either a personal or professional nature. His office door was never closed, and students and colleagues were always welcome. He showed the same concern for his graduate students as he did for his own children. He was enormously respected by those who worked for him, not alone for his own accomplishments, but even more because he was considerate and respectful of his fellow workers regardless of their status. His home was always open, and he and his wife were noted for the warmth and informality of their hospitality. Even without any of these accomplishments and attributes, he will be remembered for the example he set during his long illness, when he carried more than his share of responsibilities and faced the world with a courage found only in the truly great.

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KEY TO ABBREVIATIONS

Phys. Rev. = Physical Review
Rev. Mod. Phys. = Reviews of Modern Physics
Rev. Sci. Instr. = Review of Scientific Instruments

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